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Economic Geology

and the

Bulletin of the Society of Economic Geologists

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ECONOMIC GEOLOGY

VOL. XXXIII MAY, 1938 No. 3

MORE INTENSIVE FIELD STUDIES FOR LABORATORY INVESTIGATIONS OF ORE DEPOSITS.[1]

RENO H. SALES.

I SHALL speak on a certain phase of the subject of economic geology, which I hope may prove of interest not only to my associates in the field of practical mining but also to those engaged in the class room and in the laboratory. Although my own efforts in the past have been directed mainly toward the fascinating problem of how to find ore, I have not been unmindful of the extremely important part played in the search, by fundamentals of the science of ore deposits. I feel sure no one realizes more than I the debt owed by the mineral industry to those who have devoted much of their time and energy to the study of the many highly interesting problems of ore deposition.

If we consider economic geology in part as the application of the science of geology to the discovery, development, and exploitation of mineral deposits, we divide the subject automatically into two distinct fields of endeavor: first, the task of discovering the laws of ore deposition; and second, the work of applying in a useful way these principles to mining. A moment's reflection will call to mind the vital importance of the work of the first group, for nothing would present a more serious handicap to progress in the field than the acceptance of incorrect principles.

The field of economic geology is a broad one embracing all manner of natural resources such as coal, oil, iron, clays, nonferrous and precious metals. In such specific references as I may make to ore deposits or to geologic processes, my remarks will apply only to ores of the precious metals or to those of the

[1] Presidential address read before Society of Economic Geologists, New York, February 16, 1938.

non-ferrous group. I shall, as far as possible, draw upon my own experiences and observations. I shall try to point out the need for more detailed field studies. In stressing the great importance of intensive and accurate work on types of deposits exhibiting elements of similarity in character and origin, I make no claim for originality. The advantages of such a course of action have been advanced upon a number of occasions, and particularly by Dr. Lindgren reporting in 1928 for the Committee on Processes of Ore Deposition. I recommend earnestly that we heed Lindgren's suggestions and advice. I wish to go further and urge specifically that the geological relations of ore deposits, embracing rocks, rock alteration, vein structure, and mineralogy, receive more attention from laboratory investigators than in the past, and that accurate geological mapping of the deposit or districts chosen for study be made the basis of laboratory work. I hold no brief for mining geologists and therefore urge with equal emphasis that their field observations be supplemented by laboratory research.

In making this suggestion to you I have in mind the more obscure and difficult problems relating to ore genesis. At the recent meeting of the Geological Society of America in Washington, N. L. Bowen suggested phase relations in magmas as perhaps the most important problem for study in connection with ore deposits. I agree whole-heartedly with Dr. Bowen, but I must insist that unless a proper foundation for laboratory study be built up from facts gathered in the field, the advance sought by the economic geologist or scientist will be slow. The rate of progress will be increased in proportion to the extent to which the gaps between the two fields of endeavor in economic geology already mentioned are bridged by students of ore deposits whose investigations cover all phases of the problem. I think our recent progress toward a better understanding of structure and mineral association, has been aided greatly by the work of Graton, McLaughlin, Bateman, and many others, whose professional duties have brought them into intimate contact with the problems encountered by the mining geologist. Their connection with Universities maintaining well equipped laboratories has made it

possible for them to initiate research from a most satisfactory and favorable viewpoint.

Heated controversy arose a few years ago over the "ore magma" theory of vein formation advanced by J. E. Spurr. It revolved around the question of whether certain types of ore deposits, particularly quartz veins and their near relatives, resulted from the injection of the so-called "ore magmas," or whether the vein minerals were deposited from solutions either by direct precipitation or through replacement of the enclosing rocks. Dr. Lindgren, Spurr's chief critic, supported the latter view but J. F. Kemp took a middle ground. Out of these extended discussions the jury of non-combatants could make no final decision or findings of fact, because, in my opinion, the arguments and discussions were not accompanied by enough evidence direct from the field to be convincing. It appeared relatively easy for Lindgren or Spurr to cite examples of mineral associations in quartz veins in support of his own theories, but I think both overlooked the fact that veins may result from either process, acting singly or through an overlapping of these processes acting in chronological sequence. In such a case, either Spurr or Lindgren might find, in a single vein, abundant evidence to justify his own interpretations and conclusions. Kemp apparently recognized this possibility and his middle ground position was made clear in the following statement:[2]

> In quartz veins productive of gold, with associated sulfides, chalcopyrite, arsenopyrite, pyrite, galena, zinc blende, and perhaps some rarer sulfides, often the quartz vein has obviously been first formed and then shattered. The sulfides and the native gold have come into little cracks and fissures and now appear with irregular, triangular cross-sections in the specimen. Clearly in their precipitation, they have followed systems of cracks.
>
> The filling of such a vein and its values must then be assigned to two periods; one of quartz without much value in it, and afterwards one of enrichment by the circulation of solutions that brought the metallic elements into the cracks and deposited them.

In the above brief statement, Kemp does not attempt to assign a specific manner of origin to either the early quartz or the later "metallic elements."

The serious and valuable discussions aroused by Spurr's theory

[2] Kemp, J. F.: A. I. M. E. Trans., vol. 74, p. 95, 1926.

did much at the time to bring out the best thought on ore genesis. Those who heard or read what Spurr, Lindgren, and others had to say must have gone into the field with enthusiasm, each with a determination to do his bit for the sake of the science. Inasmuch as my own experience up to 1907, when Spurr's theory was first advanced, had been principally in the Butte district, I found difficulty in accepting the " vein dike " theory for the origin of vein deposits. I now believe it may have application in certain localities other than Butte but to a much more limited extent than advocated by Spurr. I should add, however, that Spurr's theory as finally presented was broader and more flexible than at the time it was first offered. There is but little recognizable difference between his " highly attenuated " ore magmas and the vein forming solutions of Lindgren.

In my personal endeavors in the past to put to practical use the theories of ore deposition as I interpreted them, I have met with difficulties. The reason seems to be that the proponents of the theories neglected to supply enough supporting geological evidence to enable me to test the validity and application of their theories to the problems arising in practical mining. And, thinking it might be of interest to this assemblage, I am going to discuss briefly a phase of the problem of ore deposits to which I have given some thought, in the hope that a better understanding of it might have application in that expensive investigation called ore hunting. I shall give some observations that may throw light on questions concerning vein formation, but I hasten to assure you these are offered here only for the purpose of encouraging more intensive studies in the field.

For many years I was puzzled over the presence of dike-like seams, veinlets, and masses of quartz associated with certain ores in the highly mineralized districts of Inspiration, Chuquicamata, Cananea, and others of the so-called porphyry type. The chief physical characteristics of these seams and veins, which so readily distinguish them from other quartz of the same districts, are, first, the uniform thickness of each individual seam or vein, and, second, their sharp contacts with the enclosing rocks, displaying no apparent tendency toward replacement. There is no banding

or orderly arrangement of the quartz. Sulphides, where present, show no association with either cross or longitudinal fractures, but occur as isolated grains or masses. In texture and appearance, the quartz may range from dense milky-white to an opposite extreme so glassy in texture that the term " pegmatitic " has been applied to it where associated with the ores in Cananea and Chuquicamata. At Chuquicamata there is a well defined zone containing veins of this type extending longitudinally throughout the mineralized area for several thousand feet with individual veins from a fraction of an inch to several feet in width. Owing to the fact that there had been no regional fissuring in the Cananea District at the time of the appearance of the ore minerals, quartz veining was localized within the breccia pipes, or in adjacent rocks.

In strong contrast to the dike-like massive quartz, the heavily mineralized areas of these districts also contain later quartz and metallic sulphides having structural and physical attitudes toward the enclosing rock distinctly different from that just described. These have formed from solutions that came up through fissures or other openings and spread out along seams and joint planes, and from there finally worked outward into the body of the mass of the rock. Where there was precipitation in openings or where replacement of the wall rock was complete, typical quartz stringers or veins developed showing irregularities in width and continuity, but characteristically preserving banding or center line divisional planes, marking the positions of the original fracture. Another type of silicification common to porphyry deposits, sometimes called " flood silica," develops where silica-bearing solutions invade the rocks outwardly from channelways. This produces partial or complete replacement with little or no development of vein quartz, but rock structures, such as joint planes, are preserved, and mineral textures are only in part eliminated.

In my opinion, the types of mineralization last described could not have resulted from " ore magmas " so-called, or by any vehicle incapable of penetrating the minutest openings existing in the rocks. The time element enters here, too, since it is evident that

the action was marked by a relatively slow building up of the veins and silica replacements, something scarcely to be expected in the case of cooling intrusive magmas. It is of economic significance that the solutions responsible for the "vein" and "flood" types of silica replacements in deposits of this character are the principal carriers of the metallic sulphides.

From the fact that the dike-like type of quartz, and the silica replacements with their associated sulphides I have just described, are common to all of the districts mentioned, it is clear that there was something definite and orderly in the processes through which the minerals comprising the ores were separated from the deeper igneous rocks supplying them. Orderly, too, were the processes that brought about definite mineralogical sequences and vertical range distributions in the near-surface regions of the earth's crust now accessible for study. Rarely, however, does surface erosion or mine workings expose sufficient vertical range to enable the observer to obtain the vitally important data needed to complete the picture.

Fortunately, the Colorada breccia pipe ore body at Cananea, Mexico, is one exception. Time will not permit me to describe the geology of this ore body, or to go deeply into theories of origin. It must suffice here to say that it has been given long study and it is the one ore deposit I have seen, where in effect, nature was almost caught in the act of propelling an ore mass along a zone of brecciation leading to the higher and cooler regions. It appears that at the critical moment, no escape channels extending through to the surface were available. Consequently, the ore mass was trapped on its upward journey and held within a pre-existing breccia pipe where it underwent a process of segregation, and later solidified by stages considerably below the then existing surface, but at an elevation now penetrated by mine workings. According to Perry:[3]

> Under conditions of high temperatures and pressure, the ore-bearing material segregated into aggregates of quartz and copper sulphides.

[3] Perry, V. D.: Ore Deposits of the Western States, p. 704, 1933.

And further:

> Under the accumulation of gas pressure and probably, too, because of chemical attack, the porphyry roof of the lower element finally ruptured, permitting the escape of successive surges of quartz and copper sulphides into the upper element of the pipe. In this part of the structure, the quartz crystallized as an upward expanding ore ring with a sub-surface apex at the 500 foot level.

The ore body mentioned by Perry, which approaches an ordinary goblet in shape, including walls, bottom, and stem, is composed chiefly of glassy quartz with subordinate amounts of coarsely crystalline mica, and the sulphides of copper, molybdenum, and iron. The quartz composing the walls and bottom of the goblet is 5 to 50 feet or more in thickness, but in addition to the main mass there are numberless small stringers and veinlets extending into the wall rock and into the rocks overlying the ore ring. These small quartz veins exhibit the usual structural features of dikes, such as angular zig-zag strikes and dips, sharply defined walls, uniform widths locally, and show no evidence of having been built up from a central fracture. Sulphides, where present, are studded irregularly within the quartz. There is every reason to believe these small veins were shot into the country rock contemporaneously with the movement of the main body of the quartz into its final position forming the ring. The sulphides, which did not solidify until later, remained in liquid form inside and at the bottom of the goblet. Accumulating gas pressure, together with an apparent slumping of the interior cone-shaped rock mass, forced portions of the sulphides upward along the inside walls of the goblet and outward through the quartz which had been fractured considerably subsequent to solidification. Later the copper sulphides were partly brecciated by continued slumping of the highly altered interior rock mass. It is interesting to examine the present surface overlying the Colorada ore body. Perry[4] says:

> Over the hidden Colorada ore body there are, besides the outcropping quartz porphyry, weak but significant structural features that are manifestations of the underlying structure. There is a well defined zone of narrow glassy quartz veinlets that forms a broad half-arc 100 to 200 feet wide over the north side of the ore body. These veinlets crisscross in all

[4] Op. cit., p. 706.

directions, but the stronger veinlets have a trend coincident with the curving strike of the underlying ore. Cutting these quartz veinlets are later numerous iron oxide veinlets that evidently are weak manifestations of underlying sulphide mineralization. These weak oxide veinlets are similar to the veinlets outcropping over important porphyry copper deposits in other Western districts.

The only difference between Perry's " iron oxide veinlets " and the small seams and veins of other districts I have described as having resulted from replacement, is that they contain less quartz but more iron oxide than the general average. This may be accounted for by the fact that the final Colorada sulphide mass was deficient in silica, unusually low in iron but rich in copper.

From the viewpoint of the field evidence, as I have given it, there is a marked similarity between the smaller veins and ore structures in and surrounding Colorada ore body, and those noted in connection with the porphyry copper districts to which I have referred previously. In drawing comparisons, however, it should be kept in mind that the Colorada deposit is but a small feature in a large area that has been the scene of mineralizing activity of deep-seated origin operating on a tremendous scale. Furthermore, the observations in the Colorada were made at or near the point where final separation of the ore into quartz and sulphides probably took place, whereas in the other districts mentioned the quartz veins, together with attendant alteration effects observed now occupy a zone thousands of feet removed from where the ore and gangue minerals were segregated from the liquid rock mass and began their journey toward the surface. In other words, the Colorada might be thought of as a small scale representation, a laboratory model if you will, of a process of ore deposition which may have had its counterpart on a large scale basis.

I shall not attempt here to explain the origin of these veins and seams of quartz whose structural attitudes and physical characteristics are suggestive of dikes. On the assumption that they have resulted from the sudden cooling of colloidal silica, or so-called ore emanations related to the pegmatite family, I would expect the arrangement and distribution of any contained sulphides to be different from that where deposition took place from solu-

tions acting over relatively long periods of time, and where the solubility of the ore minerals might be greatly influenced not only by temperature and pressure changes, but also by chemical reaction with enclosing rocks.

If it be true generally in the porphyry districts cited that there is a definite genetic order of deposition beginning with the leaner massive quartz types and passing through later less and less siliceous phases, but with increasing sulphide ratios in each successive surge of mineralization, the question naturally arises, what scientific or practical application, if any, can be made of these observed relations to other types of deposits genetically associated with igneous rocks, particularly where quartz is a predominating vein mineral?

I have observed frequently the phenomenon of two or more ages of mineralization in quartz vein deposits. Commonly massive quartz is first to form, followed by a second surge containing higher proportions of sulphides, these having been introduced through longitudinal fracturing within or along the massive quartz. It must be remembered, however, that the existence of two ages of mineralization in a vein does not mean necessarily two distinct separate mineral surges from the same source, because definite mineral sequences may develop from the reopening or fracturing of a vein during a single period of time, through which mineralizing processes are active.

If we conclude that a vein or ore deposit can result, first, from the cooling of an injected liquid mineral mass, consisting in the main of silica and metallic sulphides in highly variable proportions, or second, from the deposition of minerals from aqueous solutions of igneous derivation traversing fissures or other channelways in rocks, either through precipitation in openings or by partial or complete replacement of the wall rocks, then there is no valid reason why deposits should not result from a combination of these two processes acting in the same chronological sequence previously noted as common to certain porphyry deposits, and similarly deriving their mineral substances from a common igneous source. The identification of depositional factors as above outlined coupled with correct inter-

pretations of age relations, would form a most satisfactory basis for the practical analysis of many of the fissure vein types of ore deposits.

I am sure all geologists appreciate the great difficulties inherent in this problem. If we begin with the assumption that the ore minerals of a deposit or district originate as a differentiate or emanation from a deep lying rock magma, we are faced with a problem bristling with indeterminate factors, such as chemical composition and physical character of the original mineral mass. Experience in the study and examination of ore deposits known to be genetically related to igneous rocks, however, has taught me that the composition of original ore mineral differentiates must have covered an extraordinarily wide range, with silica at one extreme of the series and sulphides at the other, and with infinite variations in the silica sulphide ratios between these extremes. The silica may give way in part to other earthy minerals or the sulphide whole may be made up of one or more of the sulphides of the metals common to ore deposits. During the upward journey to regions favorable for ore deposition these differentiates, whether liquid or gaseous in form, are subject to variations in composition owing to changing temperatures and pressures, and to chemical reaction with wall rocks. The resulting deposit at or near the surface, for example, may exhibit variations corresponding to respective wall rock types. This is well illustrated at Cananea where, in certain mines, there is an extensive development of garnet accompanying ores deposited in the bedded Mesa tuffs from solutions known to have traversed limestones immediately underlying the volcanics, but in nearby mines no garnet accompanies ores unquestionably derived from the same source but where no limestone underlies the Mesa series.

I must conclude, therefore, that two ore masses identical in chemical and physical nature at the places of origin may produce widely different ore types and alteration effects in the vertical range of deposition near the surface. The surge of a single mineral, such as quartz, might well produce the simplest type of deposit, but fortunately two or more surges depositing minerals in chronological sequence are common. I say fortu-

nately, because many times we find that the initial deposit is composed of low grade quartz or pyrite or a mixture of the two, which becomes commercial only through later hypogene enrichment.

A study of mineral sequences is a natural and correct approach to the problem of phase relations in " ore mineral " differentiates. I have always held the idea that there is a sharp distinction genetically between a " dike," as this term is applied usually, and an ore vein. A dike is essentially a projected portion of the rock magma itself, whereas veins, although believed to be derived primarily from magmas, do not, whether in solid or liquid form, exhibit characteristics of either a rock magma as I visualize it, or the final rock. Under this conception the use of the term " vein-dike " seems improper and misleading, and associating the term with " aplites " and " pegmatites " does not prove anything for these are essentially cooled rock magmas.

The broad statement that ores are related genetically to igneous rocks may be made with little fear of contradiction. It is when attempts are made to apply generalizations to individual or unrelated deposits that serious differences of opinion arise. Generalizations, like averages, are often misleading and sometimes dangerous, and I urge that they be applied with extreme care even when known to rest upon sound factual background. Many of the difficulties met with in earlier attempts to classify ore deposits on the basis of genesis can be ascribed to generalizations which were insufficiently supported by accurate knowledge of field relationships. For the same reason serious errors may have crept into the more recent classifications which are based largely on temperatures and to which depth relations have been assigned arbitrarily. The natural increase in rock temperatures with depth may be only of slight moment in comparison to the highly variable thermal effects produced by invading igneous rocks responsible for the introduction of the ores, and furthermore, the vertical range of deposition may be determined more by factors related to geochemistry than by factors of temperatures.

In conclusion, it is my belief that the attack on the difficult problems of ore genesis should begin with careful and accurate

mapping and recording of all essential geologic details, including veins and structures, mineral associations, accompanying rock alterations, and igneous contact effects. With this suggestion I am sure all will agree. But, progress from field study alone has definite limitations, therefore, observations must be supplemented by laboratory investigations. I wish especially to stress the need for more research covering chemical changes in the wall rocks associated with ore deposits. This phase of the problem has received far too little attention in the past, particularly in comparison to the time given to sulphide minerals. I think it extremely important also that investigators choose districts or deposits that can be grouped together because of similarities in geologic makeup, and which present evidence of having had comparable geologic histories. Behind this suggestion lies the thought that we are searching in reality for definite clues that will lead us consistently in the right direction. A thorough study of a number of geologically similar districts may disclose wide variations in mineral associations and rock alteration, but also it will bring to light the pertinent and significant facts from which correct and useful generalizations may be drawn.

BUTTE, MONTANA,
March 1, 1938.

ORES: FROM MAGMAS, OR DEEPER? A REPLY TO ARTHUR HOLMES.*

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ABSTRACT.

The recent arresting conclusion by Arthur Holmes that lead and accompanying metals and gangue-elements of ores cannot have been derived from granitic or basaltic magmas, because of isotopic differences between "ore lead" and "rock lead," but must have come from a still deeper source is so fundamental and of such great practical importance that it should be carefully tested before being given the acceptance that its alluring qualities might encourage. The present paper examines the validity of (*a*) Holmes' initial and underlying assumption that all originally existing lead was of uniform atomic weight, the same as found for "ore lead"; (*b*) the steps leading to calculated values for granitic and for basaltic "rock lead" that differ slightly from "ore lead"; (*c*) the assumption that such differences imply complete independence of these magmas from the ore-source through all time; (*d*) consequences in the magma depth-range and in the still deeper region if Holmes' reasoning and results be applied consistently. The finding reached is that the various evidences of magmatic origin of ores carefully assembled through the decades survive Holmes' stimulating challenge without casualty. Some aspects of radioactivity are considered with respect to geological application.

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INTRODUCTION.

In view of the growing complacency with which the magma reservoir is being accepted as the source of most types of ores, the ar-

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ticle in this JOURNAL by that brilliant geological philosopher, Arthur Holmes,[1] reaffirming the venerable idea of infra-magmatic [2] derivation, comes as a stimulating and fundamental challenge. His conclusion, reached through an ingenious chain of arguments regarding the atomic weights of lead, is that most sulphide ores of hypogene character cannot have been accumulated from granitic or basaltic magmas, and must, therefore, have come from a deeper-lying source. Obviously the paper on this account alone merits the most earnest consideration. But even wider implications are also involved, since his argument appears to reach over into the very foundation of petrogenesis. If found to be valid, this offering will stand as a contribution of the first rank; if, instead, it proves to be unsound, any temporary adoption of it will have been unfortunate.

This is the kind of article which, on its very face, encourages spontaneous acceptance. It comes from a worker of high reputation; it has root in his special field of radioactive disintegration —a field in which few geologists are expert but most have faith; it deals with quantitative data carried to many decimal places and embodied in mathematical formulae and graphs—a method always imposing to the geological mind; it is expounded with persuasive clarity and expressed in terms of undeviating finality. These are the very qualities most certain to insure for so crisp and arresting a conclusion an instant throng of adherents and a long period of approving citation in the literature.[3] Offered in the world's foremost journal devoted to ore deposition, it is especially likely to influence opinion among mining geologists who, particularly aware of their own limitations in Holmes' specialty, are likely in large numbers to accept his answer regarding ores

[1] The origin of primary lead ores. ECON. GEOL., vol. 32, pp. 763–782, 1937. Since this article is a reply to that paper references to it hereinafter will be by page only.

[2] In the following pages "magmatic" is used as relating only to those magmas of the normal kinds and sources, for convenient contrast with the materials of that still deeper region from which Holmes assumes ores to have been derived.

[3] Approving citation has already begun: *cf.* Kelley, V. C., ECON. GEOL., vol. 32, p. 1005, 1937.

as presumably the right one, or at least to be put in a state of dangerous confusion.

In these pages the primary aim is to forestall too-ready acceptance of Holmes' highly speculative conclusion of non-magmatic derivation of ores by attempting to show its degree of improbability and by examining the general method that underlies his alluring presentation. Comment is extended also to the discussion of Holmes' paper by Knopf [4] who, in seeking to reconcile the radioactive evidence with the widely-held theory of magmatic derivation of lead ores, relies on a chain of assumptions which would make both the magmas and the ores dreived from the depths.

I must assume that special familiarity with the data of radioactivity is not an indispensable requisite for testing how logically consistent and complete is Holmes' treatment of his material. My lay ideas that impinge on radioactivity are incidentally offered in the thought that there cannot be too much of the geological perspective available for the interpretation and application of this essentially chemical phenomenon.

Holmes' ultimate conclusion that ores cannot have come from magmas is found to rest on three independent propositions: *I.* That all the lead existing at the earth-beginning (*i.e.* primeval lead) was of uniform character and possessed the same atomic weight, 207.21, as now found for "ore lead" of all ages—for present purpose, the test of this is its degree of probability; *II.* That the atomic weights of "ore lead" and of "rock lead" now differ, because "rock lead" has changed with time—a test here is the quantitative adequacy of the difference; *III.* That because of this difference, "rock lead" cannot have yielded a portion which corresponds to "ore lead"; in short, that isotopic fractionation is impossible—a partial test of this involves a consideration of geological processes and relationships. Knopf apparently accepts the spirit of these premises but proposes a means intended to escape the ultimate conclusion which Holmes draws from them. The following discussion is arranged under those three headings; but it is convenient to begin with the second.

[4] Econ. Geol., vol. 32, pp. 1061–1064, 1937.

Fig. 1 may aid in presentation of the argument; it is slightly simplified from Holmes' Fig. 3.

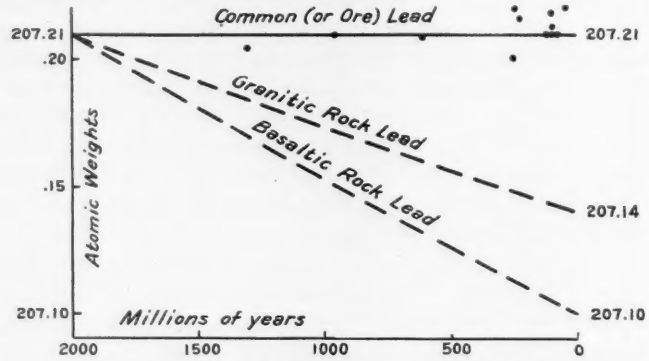

Fig. 1. Diagram showing the analytically determined atomic weights of ore leads of different geological ages (black dots and solid line), in comparison with the calculated atomic weights of rock leads for different times in the past (broken lines). After Holmes. (Note that, for ore leads, the time plotted is that of the origin of the deposit, but the corresponding atomic weight is the value determined *at the present day;* whereas, for rock leads, the time plotted is that of any past date, but the corresponding atomic weight is that which might be calculated to exist *at that same date in the past.*)

QUANTITATIVE ADEQUACY.

Standard of Precision.—Holmes takes the position that ore lead and rock lead will differ in atomic weight because the former has remained constant with time while the latter has been changing through addition of radiogenic lead. If one accepts the qualitative soundness of this position, it might he held unnecessary to examine it quantitatively; Knopf seems to adopt that view. But Holmes has quite properly regarded the magnitude of the difference as important and controlling. This difference he holds to be relatively slight, namely, 207.21 for ore lead and 207.14 for granitic [5] rock lead, a difference of 0.07 unit, or 0.07 of the weight

[5] For the sake of brevity, consideration will be limited in the main to the contrast between these two because the nature of the difference between ore lead and

of a hydrogen atom (see graph [6]). If, by favoring assumption, the maximum probable range for geological leads be taken as no more than from 207.21 to 206.0 = 1.21 units, Holmes' finding of 0.07 is about 5.8 per cent of this range. But even this 0.07 and this 5.8 per cent are maxima; they represent the difference supposed to exist after 2000 million years of accumulating divergence of granitic rock lead from ore lead. Therefore if ores were suspected of genetic association with a granite intruded at some time in the past, Holmes would have to depend on a difference in atomic weights correspondingly smaller than this 0.07; for example, if the granite were now 1500 million years old, its rock lead would then have differed from ore lead by less than 0.02 unit, or under 1.7 per cent of the maximum probable range.

Therefore, if the small difference in atomic weights which Holmes emphasizes is safely to support his sweeping conclusion as to the origin of lead ores, the two compared weights must be derived by processes which hold the eventual errors, not to this 5.8 per cent or 1.7 per cent, but to a definitely lower order of magnitude than this.[7] Apparently Holmes has either felt it unnecessary or found it impossible to adhere in each step to any such levels of precision.

Probably all will agree that the most accurate and reliable component in Holmes' entire array of data is the experimentally deter-

basaltic rock lead (207.10) is entirely similar, also because, of the two groups of rocks, those of granitic composition are more closely concerned with lead ore occurrence than are those of basaltic character. It is to be noted that Holmes classes here with the grantic rocks those intermediate types such as quartz monzonite and granodiorite with which ores are so noticeably associated.

[6] A word as to actual magnitudes may be advisable. The visual implication of the graph copied above is that the difference between ore lead and rock lead is substantial. This is due to the fact that Holmes has chosen such scales for ordinates and abscissae (one tenth of a weight-unit equals 1000 million years) that extremely slight differences in weights are strikingly exaggerated. If the scales were chosen in what might seem the "natural" balance, so that the entire atomic weight of lead, 207+, equalled the entire age of the earth, 2000 million years, the "lines" for ore lead and for granitic rock lead would plot so close together as to appear as a single horizontal line and thus be indistinguishable as to position and divergence.

[7] Reasons are given in the footnote on p. 265 for holding that the difference in weights that should be compared may be even smaller still, and therefore that the necessary level of precision is still more exacting.

mined atomic weight of common or ore lead. Even if this value be regarded as established so precisely [8] as $\pm$ 0.01, there is right here a possible variation amounting to 14 per cent of the maximum 0.07 difference in question.

On the other hand, it is a regrettable fact that not a single analytical determination of the atomic weight of granitic or of basaltic "rock lead" has ever been made. Holmes, therefore, has to deduce values for these by an entirely different and much more involved method whereby the chance for cumulative errors is far greater and the magnitude of the final error remains unknown. For example, in his computations on pages 773–4 by which 207.14 is finally reached for granitic rock lead, one can find errors of 2.0 per cent, 0.3+ per cent and 1.6 per cent besides whatever may be involved in the locating of the average line in Fig. 1 at the place shown, as well as in the inadequate decimal places in taking the respective quantities of uranium and thorium [9] and lead [10] as exactly 6, 10 and 30 parts per million, and in his other factors such as 0.36, 3.8, and 0.62.

Over and above these strictly arithmetical aberrations there stand the assumptions, approximations, uncertainties, and unavoidable errors of enormous extrapolation inherent in the observation, interpretation and quantitative expression of the radioactivity itself. It may very well be answered that the quality of the radioactive data would not justify the effort for higher levels of precision than those which Holmes has observed, as just cited; but if so, the underlying information must be regarded as unsuited for the exacting use to which Holmes tries to put it, and as failing to establish the final figure of 0.07 as valid and significant. To say this is not to belittle the value and promise of geological application of radioactivity. Its noteworthy ad-

[8] Knopf mentions the excellent agreement, within 0.003, attained by two expert analysts working independently; but this outcome on identical portions of pulp from the *same specimen* is, of course, no indication that the leads in galenas of different districts and different ages would show any such close agreement.

[9] For different average values for uranium and thorium, *cf.* Jeffreys H.: Gerlands Beit. Geophys., Bd. 47, p. 150, 1936.

[10] For indication of the approximation in the lead determination, *cf.* von Hevesy and Hobbie: Zeit. anal. Chem., Bd. 88, p. 5, 1932.

vances since its relatively recent beginnings have been achieved through progressive broadening of understanding and progressive correction of errors. As one reads Holmes' delightful and informing little book, "The Age of the Earth," [11] completed only a few weeks before his paper on lead ores was written, one cannot fail to be impressed equally by the amount that has been learned concerning radioactive disintegration and by the amount that must still be learned.

In an earlier draft of this manuscript, I undertook to set down some of the specific uncertainties from the radioactive side that enter into Holmes' present calculation; these yielded percentages of "error" such as 2+, 0.6+, 2.7+, etc. Two considerations led me to eliminate the details. The first reason is that Holmes himself called attention to the situation:

> . . . it is clear that we have to steer a difficult course through a maze of data of very variable quality, guided in some places by atomic weight and isotopic evidence; in others by well established series of accordant results; but still, unfortunately, in far too many by a subjective assessment of probabilities. Each year, however, thanks to the many investigators who are working under the *Committee on the Measurement of Geologic Time* fresh data are being contributed, and the standard of accuracy is steadily rising. (Age, 1937, pp. 174–5.)

The second reason is the accumulating indication from various investigators of new uncertainties, errors and needed corrections that quite overshadow what I could have presented and that doubtless will soon be reported in print.

Quite plainly the data to be used in geological application of radioactivity are still in a state of flux and change. Yet in his quest for the origin of ores, Holmes seems content to use these data at any given time as if they had by then been brought to a state of ultimate accuracy. It is worthy of note that, with practically the same atomic weight for ore lead as that now used, and with the data then available for calculating the atomic weight of rock lead, Holmes with much definiteness in 1926 and again in 1929 reached the conclusion that the two are so different that they could not be genetically related. In 1931, with the same value for ore lead but changed data available for the calculation of rock

[11] Hereinafter designated as "Age, 1937."

lead, he reversed his earlier position and decided (1931, p. 1040):

> Thus, in the light of the new data now available, there is no longer any serious reason to doubt the usually accepted conclusion that the lead ores have been derived from granitic and related magmas, always provided that the age of the earth is not considerably greater than 1600 million years.

Now, in 1937, with further confirmation of virtually the same value as before for ore lead, but with still other changes [12] in what he uses to compute the atomic weight of rock lead, Holmes returns more emphatically to his early stand, saying:

> The proof is complete that ore-lead cannot be a concentration from either granitic or basaltic rocks, or from their respective magmas . . . the conclusion should no longer be avoided that the lead of ore bodies such as those in which galena commonly occurs cannot have originated either by any process of magmatic differentiation or by any process of 'lateral secretion.'

One can only guess where ores will have come from when the radioactive data shall have received the next wave of corrections!

It should, of course, be acknowledged that the various kinds and magnitudes of error cited above do not necessarily cause proportionate cumulative inaccuracy in the final result. Some of the errors tend to be mutually offsetting; others are so employed that their effect is lessened, though of still others the opposite is true. But while the essential data remain so uncertain as at present, it seems necessary to conclude that the atomic weight of rock lead cannot be reached with the precision necessary to validate the use Holmes now tries to make of it.

Impropriety of Averages.—Of the many places where the use of average data is permissible, the present instance appears not to be one. In effect, Holmes implies that ore lead cannot have come from *any* magma of such composition as to be designated granitic. For if lead ore might come from certain granitic magmas but not from all, the broad conclusion that he reaches would require drastic narrowing and the geological fact he emphasizes that not all intrusives are attended by known ore could no

[12] The change between 1931 and 1937 from 1600 to 2000 million years for the age of the earth exerts an insignificant effect upon Holmes' calculation; it is changes which he did not anticipate in 1931 that have caused him now to shift from his position then.

longer puzzle him. From this point of view, therefore, it is not permissible to employ the *average* content of the granitic rocks in lead, thorium and uranium as if this typified the capacity of *all* granitic rocks to yield ore lead. To whatever extent his general conclusion may be sound, it can be strictly applicable only to those individual rocks which have the same content of lead, thorium and uranium as shown by the averages. If other rocks deviate far enough [13] from the average, they are utterly exempt from the conclusion. Any rock with a Th : U ratio of 4.3 would yield radiogenic lead averaging 207.21 and thus remain, regardless of age, identical with ore lead, according to Holmes' other arguments.

By scaling off from Holmes' Fig. 1 the values for those 10 samples that show the highest thorium : radium ratio and applying to their average the procedure used by Holmes on pages 773–4, one finds that the average atomic weight for those particular 10 samples of granitic rock lead appears to be 207.202, which is within the accepted limit of error for the atomic weight of ore lead. And if, similarly, the 20 samples of Fig. 1 having the highest thorium : radium are used, their average atomic weight appears to be 207.180, which likewise is too close [14] to the accepted value for ore lead to justify any safe distinctions. In short, it would seem that Holmes' conclusion does not apply to these 20 samples; these 20 granitic rocks *could* have yielded ore lead. Yet these 20 represent almost 1/7th of the total number plotted in Fig. 1. If one out of every seven known granitic intrusions were associated with ores of lead, the metal would probably be much cheaper than at present.

If it should happen that for these 20 samples of higher-than-average Th : U the total Pb content is also higher than average, the atomic weight would compute out higher still. And such relationship would not seem unreasonable, since there appears to be a distinct tendency for Pb, Th and U in rocks to vary in the same direction. Unfortunately, none of the three published records of

[13] As Knopf points out, Jeffreys' tabulations show that variability in the radioactive components is widest in granites.

[14] Closer still at the time these granitic rocks were intruded.

results by von Hevesy and Hobbie shows the *range* of lead content in the 58 specimens of granitic rocks analyzed—apparently they made a composite pulp and anlyzed that; therefore we have no chance of measuring such tendency in the present case. Still more unfortunately, these specimens on which the average lead content was determined are not those from which the average thorium and uranium contents were derived. Comparison of the two lots of averages is thus doubly dubious. It seems surprising that Holmes would overlook this kind of precaution, for he says in an analogous connection:

> Another important condition for accuracy, also brought out by the work of Urry, is the necessity of making all the determinations (helium, radium and thorium) on one and the same sample of rock. (Age, 1937, p. 120.)

Finally, the "averages" cannot be held reasonably to typify all granitic rocks, but only such specimens in such proportion of varieties as were available to the analyst's hand. Such imperfect "sampling" is a common defect in much geological work. It is the duty of the mining geologist, whose training causes him to abhor such laxity, to protest whenever an example arises.

It would appear, then, even if there were no other errors, that when correction has been made for the use of averages, there remains no support for the sweeping thesis that ore lead cannot come from granitic and basaltic magmas. And the use of these particular averages adds other kinds of errors to those indicated in the preceding section.

PRIMEVAL LEAD.

The first of Holmes' series of premises has to do with the primeval lead:

> At the time when the crust of the earth consolidated, all the isotopes of lead then existing must have already become mixed together to form common lead. From that time onwards radiogenic lead began to accumulate as an independent and unevenly distributed dispersion (p. 772).

If the composite isotopic character of ore lead, as indicated by its fractional atomic weight and confirmed by the mass spectrograph, implies initial derivation from various non-composite origins, does it seem reasonable to suppose that the "mixing" im-

agined by Holmes, which must have affected all materials then containing lead, was everywhere so thorough that all the lead then present in the earth acquired the now-emphasized uniformity of composition? (*Cf.* later, "Minero-chemical Considerations, 7.") Is this more reasonable than to assume that the present degree of uniformity of the lead in galena and its oxidation products is a consequence of some underlying similarity of process wherever those selective operations that resulted in a segregation of lead sulphide have been at work? And if the "mixing" of lead isotopes was so thorough and homogeneous on an earth-wide scale as Holmes imagines, does it seem consistent to assume a very heterogeneous distribution of radioactive[15] elements—which Holmes' assumptions are found to require?

Holmes repeatedly intimates that the approximate constancy in determinations of the atomic weight of ore lead despite difference in age of the ores is proof that ore lead has not changed since the earth-beginning. Knopf apparently accepts this idea. Surely this, standing alone, is no proof. The close agreement among good determinations of ore lead means no more than that all the samples tested possess *at the present day* essentially the same atomic weight. This is exactly what Holmes holds for rock lead[16] which he concludes *has changed* with time. Why the different samples of ore lead that have been tested are in such close agreement as to atomic weight is an interesting question. But whatever reason there is for thinking that they have been held unchanged with time is *not* this present-day agreement *but the admitted absence or paucity of radioactive material* in the mineral tested (galena or its oxidation products). With this understood, we may, for the time being, proceed with Holmes on the assumption that ore lead has remained approximately constant at about 207.21 throughout earth time.

But that assumption can be true only if the source from which

[15] As used throughout the present paper, "radioactive" applies only to those elements and processes which yield lead as a disintegration product.

[16] That is to say, Holmes assumes that the rock lead of all granites calculates out at 207.14 *now*, although the various granites may be of widely differing geological ages.

ore lead has been at different times derived (to yield ores of different ages) has from the beginning been devoid of elements capable of yielding radiogenic lead.[17] Holmes does not mention this qualification of the ore-source, but it seems to follow inescapably from his argument. We must then picture at or shortly after the earth-beginning two differing kinds of material: the radioactive-free material from which ores have been derived, and the radioactive-bearing material from which granites and basalts have developed. This important difference between the two kinds of source-materials with respect to radioactive content would seem to imply that they had a striking degree of independence from "the time when the crust of the earth consolidated." And such necessary independence does not seem compatible with Holmes' initial premise of thorough mixing of *all* the primeval lead.

In short, the premise that rock lead started out with an atomic weight of 207.21 seems to be not soundly established but only one presumption among many that might be made. That rock lead and the lead of the supposed deep ore-source had the same atomic weight at the beginning seems more unlikely than likely, in view of the other requirements of Holmes' argument. If rock lead did not have the atomic weight 207.21 at the outset, there is no telling what its initial atomic weight was—it may have been anything between 208 and 204—nor is there any assurance that it was of the same value at all places. In this connection, see later, under "Minero-chemical considerations, 5."

Yet, unless Holmes' calculations for rock lead can safely take 207.21 as the initial value, they become indeterminate and his present-day value of 207.14 for granites (and 207.10 for basalts) has no standing. Moreover, any granite source-material in which the rock lead was originally of higher atomic weight than 207.21 and in which the Th : U ratio was anything like the 1.7 value Holmes adopts could, at an appropriate time in its radioactive evolution, have yielded lead with an average atomic weight of

[17] Unless radiogenic lead that chances to have 207.21 atomic weight.

207.21—*i.e.* could have yielded ore lead as defined by Holmes.[18] That "appropriate time" could have been at the birth of the eventual ore deposit or at any time earlier. The only requirement would be that at the time of separation of that lead, radioactive elements would be excluded from it. Holmes has to assume such selective exclusion of radioactive elements from the ore-source at the beginning. Knopf likewise assumes them to have been negligible in the deep basic material from the beginning. Exclusion from the ore-stuff at a later date seems not greatly more speculative.

Whatever the actual facts, then, it is apparent that Holmes' premise of uniform atomic weight at 207. 21 for all primeval lead can hold only if certain other important relationships exist; yet for these no independent support is in sight. Without such assumptions (whether stated or implied), his whole argument appears to break down.

Knopf appears to accept 207.21 as the universal value for the primeval lead; for he adopts it for the deep, more basic material, and he also implies that addition of any radiogenic lead averaging lower than this would bring the resulting atomic weight of granitic or basaltic rock lead down below 207.21.

PERMANENCE OF ISOTOPIC PROPORTIONS.

The third proposition on which Holmes' conclusion rests he makes no effort to defend, perhaps because he thinks it to be self-evident. In any case, throughout his paper there is implicit dependence on the impossibility of deriving from granitic or basaltic rock lead any fraction or segregation which has the allegedly different atomic weight of ore lead. Even if the identity of ore lead and rock lead at the beginning and the 0.07 difference in atomic weights at present were established beyond all doubt, this third proposition must likewise be true if the non-magmatic origin of ores is to be established. Knopf appears to subscribe without question to this idea of constancy of isotopic proportions for lead except through modification by radiogenic additions. It must

[18] Or, conversely, a granitic source material having lead with a lower atomic weight than 207.21 and a Th : U ratio greater than 4.3.

be admitted that they have the support of preponderant chemical opinion on this question.[19] Nevertheless the subject merits examination from its geological aspects. One need not commit himself to belief in isotopic fractionation merely by setting forth considerations of that possibility; moreover, because his other two premises are so vulnerable, it is not a question of either proving isotopic fractionation or else accepting Holmes' conclusion.

Minero-chemical considerations.—1. In the case of certain other elements, including mercury (which has an atomic weight close to that of lead), it has recently become possible to effect a partial but considerable separation or fractionation of a given isotope from isotopic mixtures in a relatively brief laboratory run. It would, therefore, seem that man's inability as yet to do the same convincingly for lead is a hazardous basis for insisting that differing combinations of lead isotopes cannot be segregated by specialized processes that go on under the varied conditions maintained in and about a magma reservoir during unknown but extremely long stretches of time. It seems pertinent to ascertain how great a change of isotopic proprortions would be required if ore lead of 207.21 were extracted from granitic lead of 207.14. The following comparison is derived[20] from Holmes' own data and those he quotes from Aston. (Age, 1937, p. 143):

[19] One of the most exacting tests of lead-isotope fractionation appears to be that by Richards, King and Hall, noted in Holmes' bibliography (J. A. Chem. Soc., vol. 48, 1926). These investigators cited conflicting evidence by others. From their own experiments they concluded (p. 1542): "The separation attained was not in any case greater than the possible error of experimentation, which was very small." But since the experimental error was, after all, of substantially the same magnitude as the theoretical expectation for isotopic separation under their conditions (p. 1541), their findings did not exclude such action. And they say of their negative results: "Of course this outcome does not show that by much more extended treatment it might not be possible to effect perceptible separation."

[20] For simplicity it is assumed that the proportion of primeval lead, 207.21 (which according to Holmes constitutes 87.3% or 26.2 of the 30 parts per million of granitic rock lead), remains the same in the extract as in the rock, but that a somewhat greater proportion of the thorium lead and a somewhat lower proportion of the uranium lead are incorporated in the extract than are present in the granitic source-material. Assumptions different from this one could be used, with somewhat different percentages resulting in column *B*.

RELATIVE PROPORTIONS OF ISOTOPES.

Isotope	*A.* Holmes Assumes in Granitic Rock Lead	*B.* Possible Extract to Constitute Ore Lead
Others than 208 and 206	19.8%	19.8%
208	47.5	51.2
206	32.7	29.0
Average Atomic Weight	207.14	207.21

It thus appears that a selective extraction which at very slow rate long maintained would change certain of the isotopic proportions by not more than 10 per cent would suffice [21] to give the indicated difference in atomic weight. Is a selective fractionation of such degree a reasonable possibility under geological conditions?

2. According to present ideas, isotopes of the same element are so identical in chemical behavior that any separation or fractionation would preferably be accomplished by a method that discriminates between their different masses. Diffusion is one of those methods.[22] The growth of a given crystal from a fluid solution of more complex composition presumably involves selective transport of the wanted atoms to the growing surface chiefly by means of diffusion. As a crystal or compact group of similar crystals

[21] It is to be noted that Holmes' calculations for "rock lead" apply to the crystallized product, not to the magma. If lead ores have been derived from magmas, the lead in the igneous rock would then represent the residual fraction and the ore lead the escaped or extracted fraction. The degree of selective fractionation from the *magma* lead as a starting point would thus be less than the degree indicated in the foregoing tables. If one were to assume, for example, that the amount of lead retained in the crystallized rock approximately equals the amount that escaped by the mineralizing process (whether to form commercial ores or mere worthless dissipations), then only half as much differentiation of isotopes would be attributable to the ore-forming mechanism as is indicated in the tables and the maximum difference in atomic weights between ore lead and granitic *magma* lead would not be 0.07, but only 0.035; and still less as older and older ores are considered. This situation applies at every place throughout this paper that involves the numerical contrast between ore lead and rock lead *provided* the ore lead be entertained as a magma derivative.

[22] It is interesting to note that Richards, King and Hall did not try diffusion methods.

reaches substantial dimension the distance through which the diffusion must operate is correspondingly increased; this may remain true even though chemical transport be aided by circulation of the solution. And the grain-size in rocks, and particularly in ores, averages far greater than that ordinarily dealt with in the chemist's crystalline precipitate. Therefore, the chance for selective diffusion of isotopes (as well as of elements) must be greater in nature's laboratory than in man's. In the second place, although diffusion in solids goes on so slowly as to remain little investigated by physicists and chemists generally (though much by metallurgists), such diffusion is a process of the utmost importance geologically, especially in ore-genesis—the wide-spread phenomenon of replacement of minerals and rocks is inescapably dependent on two-way diffusion through the solid. Therefore, at many localities of replacement deposits, transportation of chemical elements by the process of diffusion must have gone on through tens and hundreds if not thousands of feet of solid rock or mineral. It would seem that the possibility of isotopic fractionation under such conditions may be far greater than in any man-made tests yet undertaken.

3. In pegmatites derived from granites, any uraninite or thorite present represents respectively a 72,000-fold and a 128,000-fold concentration above Holmes' figures of 10 parts thorium and 6 parts uranium contained in the parent granite. (If galena came likewise from a granite magma, it would represent slightly under a 29,000-fold concentration on the, say, 30 parts per million of the lead in granite).

Now, in the course of processes that are so superlatively selective as these, may there not have been also a sufficient (one-tenth-fold or less) selective or differential concentration of lead isotopes to have yielded galena lead with an average at. wt. of 207.21 from the parent granite lead averaging 207.14 or more?[22a] Certain suggestive natural occurrences are mentioned below.

[22a] In the discussion of Holmes' conclusions by Dr. R. C. Wells (ECON. GEOL., vol. 33, pp. 216–217, 1938), which appeared when the present paper was in proof, it is of particular interest to find that this able chemist, trained under Richards, has no hesitancy in contemplating differential or selective extraction of lead isotopes.

4. The galena of veins may be associated with radioactive minerals, or vice versa. In only one such instance, Great Bear Lake, does the atomic weight of the galena lead appear to have been determined, giving the value 207.205; but that galena is said to exist in a sulphide replacement band which is cut by later veins that carry the pitchblende,[23] so it is not a real test of the question at issue. In other instances, in which the lead is evidently as much a part of the vein as the associated radioactive mineral or minerals, the investigators have been content to calculate the proportion presumed to be non-radiogenic lead *by assuming* that its atomic weight is about 207.21.[24] The same is true for lead in pegmatite probably derived from granitic magma.

When galena is found in a sample of pegmatite containing radioactive material, and the galena lead is assumed to have an atomic weight of about 207.21, two alternative conclusions are compatible with that assumption: either that such galena lead has been *selectively* acquired from the granitic source of the pegmatite, or that it is a later introduction independent of the pegmatite in

In referring to Holmes' explanation for the supposed difference between the atomic weight of ore lead and that of "rock lead" Wells says that the difference "might be accounted for equally well by assuming that radiogenic lead remains fixed in the uranium-thorium minerals and is not extracted by migrating waters to form ore deposits. If only common lead is so extracted and concentrated the lead of all ore deposits should be common lead, as appears to be the case. Some difference in the solubility of the minerals might account for this difference in behavior. . . . Even if the original source had been an igneous rock, the lead would probably have been extracted by a hydrothermal solution and it would still be possible for the radiogenic lead to remain fixed. Nothing short of a drastic treatment, such as fusion or solution in acid, might be able to extract the radiogenic lead."

The final sentence quoted and the general context suggest that Wells regards ore accumulation as accomplished by leaching from rocks *already solid*—whether sedimentary or igneous; and this would presumably better facilitate his idea of differential extraction of non-radiogenic from radiogenic lead than if the process of ore derivation were more intimately connected with the *crystallizing magma,* as many mining geologists are disposed to assume. But the proposal of *any* means of chemical separation of isotopes is especially pertinent here.

[23] Marble, J. P.: Jour. Am. Chem. Soc., vol. 59, p. 653, 1937.

[24] Why wouldn't it be wiser and sounder, wherever possible, to separate such lead (when in the form of galena) and independently ascertain its actual atomic weight and isotopic composition before taking so round-about a way of trying to prove that ore lead cannot have granitic parentage?

origin. The first alternative would contradict Holmes' present conclusion. The second alternative is implicitly adopted by him, on the basis of later age of the galena as revealed by microscopical investigation. But galena is well known to be one of the latest of the hypogene components in an ore or pegmatite, and especially late as compared with oxides and silicates of the heavy metals, including such as thorite and uraninite. I have examined well-polished surfaces of most of the radioactive specimens investigated by Baxter in recent years, and the galena in these appears to be as much a member of the genetic assemblage as is the radioactive component. If, then, the assumption that *this* galena lead is of 207.21 at. wt. is verified by determinative test, it will be difficult to deny that such lead when in pegmatites can come from granitic magmas. And if the test of such (pegmatitic galena) lead yields any other value than one very close to what Holmes would compute for granite of corresponding age, he would still apparently have to admit that selective segregation of lead isotopes from a granitic source is geologically possible; and on this admission likewise the proposition that ore lead is not of granitic parentage would break down.

5. Whether or not the lead of galena that is in intimate association with radioactive minerals (in either pegmatites or mineral veins) may be found to have an atomic weight different from about 207.21, it seems rather surprising that no other unquestioned *ore* mineral of lead except galena and its oxidation products has been tested as to atomic weight.[25] A dozen or more well crystallized and thoroughly authenticated lead-rich sulphides and sulpho-salts, as well as the telluride and the selenide, suggest themselves for such test. Unless each of these should show substantially the same atomic weight as does galena, the reality of isotopic fractionation by the ore-forming process would be established sufficiently to nullify Holmes' conclusion. Especially significant would it be if different lead minerals in the same ore were

[25] The overwhelming preponderance of lead from galena or its oxidation products in all available examples of the commercial metal prevents the drawing therefrom of any safe inferences about the lead of other minerals.

found to give different atomic weights and different isotopic proportions.

I had thought that the natural $PbCl_2$, cotunnite, the sublimate from Vesuvius mentioned by Holmes, might constitute an example of the ore-forming process yielding an isotopic fraction with a lower-than-average atomic weight, viz. 207.05—as would be expected in a sublimate or distillation product. Fumarolic sublimates of this kind and similar products and manifestations associated with volcanic activity have, with ever-increasing unanimity and evidence during the last hundred years, been regarded as surficial equivalents, or at least intimate relatives, of the typical ore-forming mechanism. It seems strange that Holmes ignored such interpretation of the cotunnite and has concluded, instead, that it is a " naturally isolated sample of rock lead " yielded by the rather unusual leucite-tephrite magma of Vesuvius. But Professor Baxter informs me that the method used by Piutti and Migliacci was such that, when recomputed according to datum (atomic weight of carbon) revised since their work was done, their check for common lead becomes 207. 8, instead of 207.19 as they supposed. This means either that their results cannot be given acceptance or else that common lead is not of constant atomic weight. In either case, their finding of 207.05 for the cotunnite becomes too questionable to be given dependence. Thus the sole example of what could possibly be interpreted as a *determination* of the atomic weight of rock lead is cancelled and hence is lost what Holmes regarded as its " great value in confirming the inference here drawn, that, on the average, the atomic weight of rock lead is significantly lower than that of common lead."

6. It is not to be overlooked that, in the case of pegmatites and veins, the grain-size is coarse enough to permit individual minerals to be separated and tested; and, so far as I am aware, nothing more than this has ever been done in connection with the lead-age method. For example, in a pegmatite or a vein we know the facts about some particular radioactive mineral component, but not about the pegmatite or the vein as a whole. In any attempt to deal with rock lead, on the contrary, the minute quantity of total

lead has thus far necessitated that only the aggregate in the rock be dealt with, even in calculations. No real information is available as to how the lead disclosed by analysis occurs in the rock. Neither is there more than a tacit presumption based on "freshness" that the lead present is an original and inherent part of the rock.

The fact is important that in many rocks metals occur as sulphides, even in localities where no ore deposits are known. Newhouse [26] has described a considerable number of such occurences which he interpreted as belonging in the strictly magmatic stage characterized by crystallization of the rock. But there are also numerous examples of "fresh" rocks in which the exacting microscopical study of both thin and polished sections disclosed minute quantities of sulphides that have been epigentically introduced; these, in fact, constitute minature ore deposits. Among the 58 samples of granitic rock which von Hevesy and Hobbie analyzed, if a single one of these samples had contained in each gram enough tiny scattered particles to aggregate the equivalent of a cube of galena less than 0.3 mm. on a side, this one sample would account for the whole of the 30 parts per million of lead found in the entire 58 specimens. Therefore, unless chemical data on lead content in rocks have been coupled with extremely searching microscopical study that fails to find slightest trace of galena or other lead mineral, there must remain open the possibility that the lead which Holmes is trying to prove cannot be the source of ore lead *is in itself introduced ore lead,* at least in part. From the lack of any reference thereto, it seems a fair inference that the 58 granitic and the 68 basaltic rocks for which lead averages were reported by von Hevesy and Hobbie were not subjected to any such rigorous microscopical search.

7. The lead of galena (and its oxidation products) is known to be a mixture of isotopes of differing weights combined in such proportion that the average approximates more or less closely to 207.21. It has been commonly assumed that this approximate constancy in atomic weight implied also a corresponding con-

[26] Opaque oxides and sulphides in common igneous rocks, Geol. Soc. Am. Bull., vol. 47, pp. 1–52, 1936.

stancy of proportion among the component isotopes. But recently A. O. Nier, working at Harvard with the mass spectrograph on various samples of ore lead, secured results indicating isotopic variation instead of isotopic constancy despite essentially agreeing average atomic weights. If this were definitely established, it would be of the utmost importance, since then we should apparently be obliged either to conclude that isotopic fractionation is somehow possible in the course of accumulation of galena, or else to set up special, otherwise unsupported, assumptions of different kinds of suorces for lead or varying mixtures from unlike sources; and in any event we could no longer point to constancy in atomic weights as arguing for *thoroughly mixed and uniform* primeval lead.

Just as this manuscript goes to press a summary statement of Nier's revolutionary results, now carefully confirmed, has appeared in print.[27] His tabulated findings are reproduced below; added in the right-hand column are the average atomic weights as carefully determined on the same specimens by Baxter or his students.[28]

ISOTOPIC VARIATION IN ORE LEADS.

Mineral	Geological age in M. Y.	Relative Proportions of Isotopes				Avg. At. Wt.
		204	206	207	208	
Galena, Gr. Bear Lake	1300	1.00	15.9	15.3	35.3	207.205
Cerussite, N. S. W.	950	1.00	15.9	15.3	35.3	207.21
Galena, Germany	240	1.00	18.1	15.6	37.9	207.21
Galena, Joplin, Mo.	230	1.00	21.6	15.8	40.6	207.22
Cerussite, Wallace, Ida.	80	1.00	16.0	15.1	35.3	207.21
Wulfenite and Vanadinite, Arizona	25	1.00	18.4	15.5	38.2	207.22

In commenting upon these results, Nier says:

A possible explanation for the variations is found if we consider the two oldest samples as primeval lead and the others as primeval lead con-

[27] Variations in the relative abundancies of the lead isotopes, Am. Phys. Soc. Bull., vol. 13, p. 17, Feb., 1938.

[28] These six specimens are among the 13 cited by Holmes (p. 779) as supporting the conclusion that the average atomic weight of ore lead is substantially 207.21. The average atomic weights computed from Nier's proportions of the isotopes show a somewhat wider range (*i.e.*, about 207.18–207.22) than those determined chemically.

taminated by uranium and thorium lead. As the minerals listed are essentially free from thorium and uranium, this may mean that the lead in them became contaminated before the mineral was formed.

The constancy in proportions shown by the two oldest specimens listed, which causes Nier to entertain the idea that this proportion represents "uncontaminated" primeval lead, is almost duplicated by next to the youngest specimen. Therefore, it would seem either that Nier's suggestion of constant primeval lead does not hold or else that some special reason must be found why the Idaho lead had almost completely escaped "contamination" through nearly the whole of earth time. An explanation perhaps as plausible as the one suggested by Nier is that there was no necessary constancy at the beginning and that the varying proportions now shown represent initial variations on which may have been superimposed more or less important changes through selective addition or subtraction in the intervening time. In any case it would appear that no longer can agreement in the conventional atomic weight determinations be used as an index of similarity of the leads.

The isotopic variations which Nier finds, using the 204 isotope as the datum for comparison, are shown below:

RANGE IN ISOTOPIC PROPORTIONS.

204	*206*	*207*	*208*
Datum	29.6%	4.6%	15.0%

Comparison of this 4 to 30 per cent range actually found in ore lead with the much smaller range that would be required to derive ore lead from Holmes' granitic rock lead (*cf.* table on p. 265) would suggest that even if Holmes' value of 207.14 for granitic rock lead could be taken as valid, ore lead at 207.21 could be derived from it by selective differentiation of isotopes less extreme than is actually found in natural ore lead. Obviously Nier's findings do not prove that such differentiation or fractionation has taken place, but they probably do greatly increase the plausibility of such action in nature.

Nier arrives at the following position:

> In view of these results, the conclusions of Holmes may have to be modified, inasmuch as he used the alleged constancy of common lead as an argument against derivation of lead ores from acid or basic magmas.

If there were no other reason for questioning Holmes' conclusion, this one lot of new quantitative determinations would seem sufficient to rupture Holmes' chain of argument.

Radiogenic Considerations.—1. Holmes brings into view a very important and apparently undeniable fact:

> Although a rock, as such, may be of any geological age, it is the *material* of the rock with which we are concerned, and the age of this material for the purposes of the present problem is that of the age of the earth (p. 772).

In other words, in this "material" radiogenic lead has been developing at a predestined rate since the earth-beginning. It is on this foundation that Holmes calculates the proportion of radiogenic lead in rock lead, and thereby the supposed average atomic weight of rock lead. But when the *geological* age of a granitic pegmatite is sought by means of the lead-uranium ratio, *all its radiogenic lead* is assumed to have been formed since the birth of the pegmatite as such.

This last assumption would appear either (1) to embody an erroneous neglect of the radiogenic lead that had been produced in the source-material during the period preceding the birth of the pegmatite—in which case correction of that error would completely rob the lead-uranium method of the ability to determine geological age, or else, (2) to involve the further assumption that, in the process of segregation of the pegmatite from its granitic source-material, there has been a selective inclusion of the isotopes that constitute whatever common lead may be present, and in any event a definite exclusion from the pegmatite of all the radiogenic isotopes previously formed in the granitic source-material. Either horn of this dilemma is serious. Those expert in this realm must do the choosing, or else as an alternative present disproof of Holmes' assumption about the earth-old age[29] of

[29] If Holmes is correct in his postulate that radioactive disintegration starts to affect the material at the earth-beginning, any later tranformation of that material

the "material." Wherever the inconsistency may lie, the argument that ore lead and granitic or basaltic rock lead cannot be genetic relatives seems to lose enormously in force.

2. Might not some or all of the recently synthesized "trans-uranium elements" of short half-life conceivably have once been present in the earth's crust, but no longer exist (at least not in quantities as yet detected), being thus now in the condition in which actino-uranium will presumably be eventually? If this can be entertained with any degree of seriousness, might it not also be possible that some or all of these elements could, on disintegration, yield one or more of the isotopes of lead? In that event, such lead might vitiate to an unknown degree both the present calculations of atomic weight for rock lead and the ages now being deduced by the lead method.

Petrological Considerations.—1. Led by his argument to adopt the thesis that ores come from below the depths assignable to granitic and basaltic magmas, Holmes naturally looks with interest to the peridotitic layer which many geologists presume to underlie both. From such data as are available he calculates a value for peridotitic rock lead that is much closer to ore lead than are either of his granitic or basaltic rock leads. With commendable conservatism and fairness he dismisses this indication because based on insufficient data; and with commendable understanding of the geological probabilities he gives (p. 780) the further reason for dismissal:

> The point is, however, of little more than academic interest, since there is no evidence, either from the rock-associations of lead ores or from the ore-associations of peridotites, to suggest any direct genetic connection between lead ores and peridotites.

It is therefore all the more surprising that Holmes assigns such

(*e.g.* to constitute a new intrusive rock) would probably tend to incorporate in the new rock a larger proportion of non-volatile than of volatile components. Thus, U, Th and Pb (whether radiogenic or not) existing in the old material at the time of transformation would probably be embodied in the new rock; but some or all of the radiogenic helium might escape. Thus the "helium age" of the transformed rock might be lower than the "lead age." That the helium ages are being found younger than the lead ages might thus be explained in part. To whatever extent this action may go on, the helium age would be nearer to the *geological* age and the lead age nearer to the *earth* age.

slight weight to the many geological evidences of direct genetic connection between lead ores and (certain) granitic intrusive bodies—using granitic in his broader sense. At the very start of his paper he recognizes the strength of what he calls "the circumstantial evidence" in favor of such connection; but he allows a circuitously-derived 0.07 of one weight-unit to convince him that all this geological evidence is either fallacious or fortuitous.

2. If so much dependence can thus be placed on a slight difference in atomic weights, one is led to pursue these slight weight differences as far as possible. Some of the logical difficulties that would confront Holmes in relating the radioactive components of pegmatites to their granitic parentage have been intimated above; yet the actual relationship of pegmatite to granite is unchallenged. The evidently close genetic connection between the pegmatite stage proper and the later "hydrothermal stage" which certain pegmatites disclose; the notable similarity as to process and product between this late-pegmatitic stage and the conventional ore occurrence; the instances in which pegmatites are observed, along strike or on dip, to pass unbrokenly into mineral veins, some of them ore-bearing—these are particular pieces of the "circumstantial evidence" that bring the question of relationship between magmas and ores into especially close contact with radioactivity. Is Holmes or any one else prepared to throw overboard these significant geological connections between ores and magmas for no more reliable and compelling reason than Holmes now proposes?

The indicated maximum difference between ore lead and granitic rock lead is 0.07; the corresponding difference between granitic and basaltic rock lead is 0.04. These differences are of the same order of magnitude (see graph). The numerical advantage of the 0.07 may not be great enough to offset the fact that, whereas it is reached by subtracting an indirectly and complexly calculated value from an experimentally determined value, the 0.04 difference is between two values calculated by identical procedures. It must follow that whatever conclusion may safely rest on the 0.07 difference must also presumptively rest with nearly or quite equal force on the second or 0.04 difference.

Therefore, if we join Holmes in denying that ore lead can have been derived by any conceivable process from granitic rock lead, and hence deny that ores can have been derived from granitic magmas, we seem to be led by analogous argument to the astounding proposition that, because of slight indicated differences in their leads, *granitic and basaltic magmas must have remained genetically independent since the earth-beginning!* In short, our present-day hypotheses of magmatic differentiation in the broadest sense, our entire concept of petrogenesis, would be seriously embarrassed or rendered suspect by consistent application of Holmes' theory. Must we, then, imagine that every intrusive body for which roundabout calculation shows a rock lead differing by a few hundredths in atomic weights from that calculated for surrounding intrusives must have come from some source kept independent from these others since the beginning of earth time? Must we think, for example, that the leucite-tephrite family at Vesuvius has through all time remained "isolated" and aloof, in order that its (supposedly peculiar) rock lead may have remained unique?

If a difference of 0.07 units is enough to prove no consanguinity, how much smaller must the differences become before consanguinity would be admitted? This question has pertinence to the fact that Holmes' data have been derived from rock *averages,* as well as to the fact that the difference is supposed to increase with time. Shall we say that individual rocks within the averaged granitic group *may* have a common ancestry if their leads differ by no more than, say, 0.02 or 0.03; but that there must have been genetic independence through all past time for those whose leads are as much as 0.04 or 0.05 apart? Surely any such conclusion would seem unjustifiable indeed.

3. Holmes apparently accepts the existence of magmatic processes for accumulating subordinate magmatic components, including the thorium and uranium, into much richer concentrations. He also admits the presence of lead in ordinary magmas —*e.g.*, in granites three to five times as much of it as of thorium and uranium. His argument as to a *different* source of ore lead,

207.21, would be greatly strengthened if he could point to concentrations of lead having such atomic weight as would harmonize with his calculations for rock lead. Absence of such known lead occurrences places him in the difficult position of holding that magmatic processes which effect important concentrations of many other minor magmatic components are strangely inoperative when it comes to concentrating the magmatic lead and associated ore-forming metals.

4. Holmes assigns inability of yielding lead ores not only to the magmas and magma-processes directly but also to lateral secretion from their derivatives. That "lateral secretion" as commonly understood can produce real results is not to be doubted. But my own observation causes me to join those who hold that this process, especially when accomplished by meteoric waters, is far too inefficient *in selectivity* to achieve the segregation of commercial ores from surrounding rocks that contain only minute quantities of the corresponding metals. One might therefore be tempted to cite Holmes' conclusion (p. 780) that ore lead cannot be a concentration from sediments derived from ordinary igneous rocks, couple it with the 207.22 value for galena from Joplin, Missouri,[30] and assert that here is another example to be added to the list [31] of internal minero-chemical evidences that the Mississippi Valley ores are of the same hypogene origin as is now established for most other deposits of the precious and semi-precious metals. But this does not mean that, if lateral secretion *could* form ores, Holmes' would be sound in concluding that any lead therein having at. wt. 207.21 must have come from non-magmatic material. In the course of the repeated reworkings, leachings and diffusions that attend the accumulation of sediments from igneous masses and that would be involved in a laterally secreting mechanism, one is not justified in insisting that whatever lead might be so gathered together must comprehend exactly the same proportion of isotopes as existed in the source rocks or their parent magmas.

[30] Baxter and Grover: Jour. Am. Chem. Soc., vol. 37, p. 1058, 1915.

[31] *Cf.* Spectrographic evidence on origin of ores of the Mississippi Valley type. Econ. Geol., vol. 30, pp. 800–824, 1935.

5. The "geological support" (p. 765) which Holmes says can be offered for the conclusion that ores come from deeper than the level of common magmas is as follows:

> certain crustal belts (whether igneous or sedimentary) are richly accompanied by ores, whereas others, or parts of the same belts, are disappointingly lacking in mineral wealth.

If metals were much more abundant elements than we have reason to believe they are, the erratic distribution of their loci of marked concentration might seem strange. But when we realize that an ore of commercial tenor is produced only where the effects of many favoring factors are fortunately superimposed, it is no more surprising that not every igneous intrusive is accompanied by "mineral wealth" than that not every pegmatite carries conspicuous quantities of, say, uranium and thorium, which are of about the same average scarcity as the ore metals.

6. Holmes says on p. 765:

> The common association of ores with igneous rocks is ascribed to the fact that intrusions rupture the crust and so provide fractures and fissures through which the ore solutions can readily ascend.

If this mechanism can be depended on for granitic intrusives supposed to originate above the ore-source, does it not seem surprising that intrusions of peridotite from (or at least from nearer) the ore-source should be so rarely associated with ores as an earlier quotation from Holmes admits?

And by what process shall we imagine these deep "ore solutions" to have been separated from the peridotitic or other stuff of the deep ore-source? The production of a volatile-rich mineral-bearing residue by crystallization of bodies of ordinary magma when intruded into upper, cooler rocks is the mechanism of separation favored by the majority of those who believe in magma-derived ores. Are we safe in assuming analogous crystallization at great depth, where the opportunity for loss of heat must be far less?

What is the solvent or dominant fluid medium in these deep "ore solutions"? Dare we count on any such content of volatile components in this deep, basic stuff as is known to constitute the emanations from granitic, monzonitic and similar magmas?

In the case of granitic and similar magmas, it is reasonable to associate the intrusive operation with the production of "fractures and fissures" in the *overlying* rocks since these are surely crystalline and are progressively more brittle upward, because cooler, and because upward lies the one sure direction of relief of pressure. But is it reasonable to assume that intrusions at the levels assignable to granites can cause fractures to extend for the necessary distances far enough *downward* to tap the ore solutions there assumed to exist? Would not the increasing pressure downward and the more plastic or possibly even molten character of the material of that sub-magma horizon discourage or prevent the formation of deep-reaching fractures in it?

If such downward-reaching fracturing be granted as a catastrophic accompaniment of intrusion of ordinary magmas, is it safe to assume that the imagined deeper "ore solutions" are brought into existence at just the lucky time to utilize this fracturing for their upward escape? Or must we assume, instead, that these deep "ore solutions" are always existing down there, separated in some manner from the more rocky material so that they alone can escape whenever fractures from the magmatic intrusions above reach down to their level?

Unless reasonably satisfying affirmative answer may be given to questions such as these, the single shaky argument regarding atomic weights seems entirely inadequate justification for the imagined deep derivation of the ore solutions and their ascent through fractures produced by intrusions at higher levels.

7. Holmes assumes (p. 772) that the source-material for the magmas of the kind that yield our known igneous rocks began with the primeval average 207.21 for all its lead. It is evident that he assumes this same average to exist in his imagined deeper-lying source-material for ores. The *only foundation* for these assumptions is the inability otherwise to explain the approximately constant present-day atomic weight of the lead in galena of different ages. Because he believes that the weights in the rock lead and in the deeper ore-source now differ, he concludes that the two kinds of material must have remained separate since

the outer crust solidified. But his reason for their differing is that the primeval rock lead had radiogenic lead progressively added to it. As earlier indicated, he is thus forced, consciously or not, into the further *ad hoc* assumption that the imagined deeper-lying material has never contained radioactive material capable of generating lead. On the contrary, the existence of ores containing radioactive minerals seems definitely to contradict such an idea. Thus in seeking to dispose of his atomic weight puzzle by pushing it down into the deeper and inaccessible unknown, he is confronted there by difficulties that keep the puzzle as unsolved as before. *The mysterious depths are no more capable than the known magmas of yielding "ore lead."*

8. As a proffered reconciliation between the arguments underlying Holmes' proposed derivation of ores from the deeper regions and the abundant evidences that link ores genetically to common magmas, Knopf presents an interesting deviation from certain of Holmes' ideas. He appears to accept Holmes' reasoning that magmatic derivation of ore is impossible for any cases in which the magma or its "material" had separated from the deeper, basic layer a considerable length of time before the ore came into being, since in that intervening time radioactivity should have changed the atomic weight of the magma lead from 207.21. But he avoids applying this prohibiting conclusion to many granitic and basaltic magams by proposing that they or their "material" did not separate, wholesale, from the deeper basic stuff at or about the beginning of earth time, as Holmes assumes, but individually at varying later times of petrogenic activity whereby no appreciable interval would be available for radioactive change of the lead concomitantly deposited in ores.

As I understand Knopf's proposed chain of events, it may be epitomized as follows:

(*a*) deep-seated peridotitic source-material containing common lead but *negligible* radioactive matter suffered local selective fusion to yield

(*b*) deep, basic material (less basic—and presumably less deep?—than the peridotite) from which was derived

(*c*) basaltic magma, from which was differentiated
(*d*) granitic magma, containing lead and *appreciable quantities* of radioactive elements, from which magma there separated
(*e*) the ore of lead (and other metals);
(*f*) and the entire compositional evolution and upward migration from peridotite to ore required so short a time that no appreciable quantity of radiogenic lead was produced to change the atomic weight of the lead incorporated in the ore deposit from the initial value of 207.21 held by the peridotitic lead.

We need not here be concerned from the broad petrogenic standpoint with the merit of this proposed evolution and migration so largely adapted from ideas very cautiously and tentatively advanced by Bowen.[32] But in its application to the present problem this concept seems to encounter logical difficulties especially in the quantitative direction. For Knopf couples this evolution from peridotite to granite with a progressively increasing proportion of radioactive elements. Can this fail to imply that somewhere there must result the opposite pole, that is, a sufficient (relatively great) volume of product *enough leaner* than the source-peridotite[33] in the radioactive elements (and probably in the sialic components also) to balance the postulated enrichment of these elements in the granite? Where is that product, and what is its nature?[34] Until those questions can be given reasonably definite and satisfying answers, it would seem that Knopf's

[32] The concluding sentence of Bowen's discussion of these ideas is as follows: "The main purpose . . . is not, however, to express advocacy of any particular mode of derivation of magma but rather to indicate how inadequate are the data now available to permit a definite decision on this point." (The evolution of igneous rocks, p. 320, 1928.)

[33] It is noteworthy that one of the commonly-advocated sources of heat for the selective fusion of peridotite which Knopf postulates is disintegration of radioactive elements!

[34] Goldschmidt (Metallwirtschaft, Bd. 10, p. 265, 1931) assumes that any such heavy fraction would sink toward the earth interior. Jeffreys (Gerlands Beit. zur Geophys., Bd. 47, pp. 167–8, 1936) apparently has the same idea when he speaks of "precipitation of the ultra-basic rock." Thus no test can be made of the validity of these dizzy pyramidings of speculations.

mechanism of deriving ores from the depths by way of the magmas in a manner to escape Holmes' supposed limitations of radioactivity is about as speculative and uncompelling as the one directly favored by Holmes.

EPILOGUE.

Holmes was justified in recognizing that the conclusion he had reached specifically for lead probably extends likewise to many of the other elements of hypogene ores, since the intimate genetic association of galena [35] with other metalliferous and gangue minerals is far too soundly established to permit doubt. He makes no mention of the fact that consistent application of the argument underlying his conclusion would challenge also the far broader concept of petrogenesis by magmatic differentiation, besides carrying with it unwarranted implications regarding the crustal depths. Adoption of his conclusion would thus necessitate abandonment of a vast organization of ideas regarding metallogeny and petrogeny painstakingly built up and tested through long years of thought and observation by countless workers in many lands. No one will be justified in such abandonment unless thoroughly convinced of the reality, precision and sure significance of those small differences in atomic weight, 0.07 and 0.11 or less, on which Holmes relies. It would appear, however, that he has failed to establish the validity of those small differences, as well as the value from which the divergence is supposed to have started, and has likewise failed in the logical requirement of proving that isotopic proportions in the materials he discusses could not have undergone slight change by geological processes of a selective nature.

Almost any one of the many lines of evidence that relate hypogene ores to magmas seems to me profoundly more impressive and compelling than this single, roundabout, and highly speculative conclusion of opposite import reached by Holmes. When

[35] The conclusion must also apply to many ores not containing lead, at least not at the horizon of observation; for if the only distinction between magma-derived and non-magma derived ores were that the latter contain lead, the status *ad absurdum* would surely have been reached.

one considers the mutually-supporting character of many of these independent evidences of consanguinity between ores and magmas, it must be concluded that this doctrine is impregnable against all but the most powerful assault. The present attack cannot hope to weaken the structure so solidly built on the geographic, temporal, structural, thermal and chemical affiliations of ores and intrusive bodies. The growing number of instances of preferred association of given metals (and certain other elements of the ore) with intrusives of certain petrologic types; the increasing support from both positional and physicochemical evidence for the concept of systematic zonal relations, vertically and laterally, of the mineral-bearing aureole to the intrusive body; the demonstrated existence of a mechanism of magma-caused mineralization, *i.e.* contact-metamorphism, and the unnumbered instances of contact-metamorphic ores; the undeniable examples of ore as a syngenetic component of pegmatites; the instances of direct segregation of ores from molten magmas—these are among the considerations which Holmes would have us disregard as mere "circumstantial evidence."

Holmes has done a service by indirectly reminding the mining geologists that what we have done in establishing the genetic relationship of ore to the magma reservoir is not, after all, necessarily discovering the ultimate "source" of ores. It may very well be that the magma is only a more or less temporary and way-station repository for the metals in the course of their destined migration from place of first origin or collection to place of eventual deposition as ores. The stimulation of thought on that question is both legitimate and desirable. But in my opinion neither Holmes nor Knopf has contributed toward the answer more than a highly imaginative, incomplete, and partly self-contradictory chain of assumptions. If the magma reservoirs such as have yielded the known igneous rocks are not the ultimate source of the ore materials, then, by elimination, it seems most reasonable to imagine that this source must lie deeper still. But any such inferential notion appears to gain no independent support from the recent proposals.

It seems strange that an idea of such great potential significance as the super-deep origin of ores, which Holmes has held, on and off since 1926, should not have been explored and elaborated by him further than with respect to atomic weights. His explanation of mere mechanical or structural fortuity with common igneous intrusives (p. 765) not only would demand a lot of special assumptions to allow it to work but also fails utterly to account for the host of further coincidences of temperature, substance, and process that in countless localities characterize the magma: ore combination.

Let it not be supposed that I seek to prove that ores (or granites) cannot have been derived from the deeper crustal regions. My purpose is merely to emphasize that at present we gain little of assistance and nothing of compulsion toward such derivation by the radioactive approach. In my opinion, that highly interesting method must wait for application to *such* problems until the radioactive data have become far more precise and dependable than they now seem to be. And even then, these data must be employed in a setting of the utmost logical rigor because of the colossal extrapolation that is as inescapable in applying them as in deriving them. Indeed, extrapolation in derivation is far safer than extrapolation in application. For instance, in extrapolating, on an unbelievably minute quanity of radioactive material, from the count of a few scores of alpha-rays *per second* to half-lives of *thousands of millions of years,* the weights and counts are *directly measurable* within some limit of error, so that any error involved is *quantitative only;* and our conception of geological processes and relationships suffers no revolutionary or dangerous change if, on the basis of half-lives thus obtained, earth age be estimated at 1600 million years, as it was a few years ago, and then revised to the present estimate of 2000 million or more. But when, from an *indirectly-deduced* difference in atomic weights amounting to only a very few hundredths of the weight of one hydrogen atom, we attempt to extrapolate *in different terms,* such as sources, depths and processes, the error may be enormous and will be *both quantitative and qualitative;* therefore

any error present has profound influence on geological ideas—as witness Holmes' successive shifts of belief respectively away from, back to, and again away from a magmatic source for ores, occasioned only by minor revisions of the radioactive data.

Whatever may be the ultimate fate of Holmes' specific conclusion about ores, there can hardly be doubt that his paper will arouse a new wave of thought regarding the geological applications of radioactivity. This cannot fail to be salutary and constructive, nor to justify fully the writing of his paper. Consequences of a geological kind that may be expected sooner or later to grow from this stimulation include:

fuller and more reliable understanding of the geological setting and significance of geological materials selected for radioactive examination;

more rigorous and effective selective sampling of such materials;

searching microscopical, spectrographic and x-ray examination of the materials, in the light of modern theories of mineral constitution, textures and alterations, before radioactive investigations are started;

comparative study of different minerals in the same assemblage, such as galena and bournonite from the same vein;

isolation of sufficient quantities of rock lead to permit actual determinations of its atomic weight and of its isotopic composition;

comparative atomic-weight and isotopic analyses of lead minerals of all available ages and from widely different geological environments, ranging from additional examples like the Vesuvius sublimate—*e.g.*, the fumarolic deposits at Ten Thousand Smokes, Alaska—through various oxidation products, to the several hydrothermal categories, pegmatitic ores and magmatic segregations;

increased use of isotopic analysis to supplement the atomic weight averages (and it would seem wise to test the isotopic constancy of other metals in ores even though these be not entangled with radioactivity);

finally, the best tests that ingenuity can devise for ascertaining whether isotopic fractionation is possible under the conditions of time and environment that exist within the earth.

In preparing this paper I have benefited greatly from discussions with my colleagues, who have contributed ideas that I should have missed, and indicated mistakes that I should otherwise have carried into print. I am especially indebted to G. P. Baxter, R. A. Daly, E. B. Dane, Jr., Russell Gibson, N. B. Keevil, D. H. McLaughlin, and A. O. Nier. I am also under deep obligation to Dr. Adolph Knopf of Yale, first for the privilege of seeing his discussion in advance of its publication, and particularly for his goodness in reading this manuscript and suggesting changes that have improved it. Finally, I gladly record the fine scientific spirit and generosity of Holmes himself for reading this screed and helping me with it at numerous places.

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CAMBRIDGE, MASS.
February 12, 1938.

GEOLOGY OF THE SAVAGE RIVER, MD. DAM AND RESERVOIR SITE.[1]

EDWIN B. ECKEL.

ABSTRACT.

The history of the Savage River dam site investigations is described as an example of the applicability of geology to engineering work even in places where the geology is very simple. The earth dam will rest on a series of well-consolidated beds of sandstone, sandy shale, and limestone of Paleozoic age. The beds are unfaulted and dip downstream at low angles. The water table is at or close to the surface throughout the reservoir area. Exposures are good; the zone of weathering is shallow.

The factors that influenced the choice of site are presented in detail. The presence of a large alluvial cone along one bank of the stream narrowed the choice of available sites. Toward the close of the drilling campaign small caves were discovered in the limestone in which the spillway will be cut and which will form part of one abutment. The discovery caused the inclusion of an extensive grouting program in the design.

Considerable quantities of shale will be removed from the spillway excavation. Largely as a result of geologic evidence, it was decided that this shale would prove sufficiently durable for use in the dam embankment. The decision led to important changes in design and will result in savings many times as great as the cost of the geologic investigations.

INTRODUCTION.

THE site of the proposed Savage River dam and reservoir is located in Garrett County, western Maryland, about 5 miles northwest of Westernport and 22 miles southwest of Cumberland (Fig. 1). The Savage River is a southeasterly flowing tributary of the North Branch of the Potomac River and has a drainage area of about 115 square miles. The watershed above the proposed dam is about 27 miles long, and has a maximum width of 6 miles and an area of about 105 square miles.

The purpose of the project is to provide for storage of water

[1] Published by permission of the District Engineer, Corps of Engineers, and the Director, Geol. Survey, U. S. Dept. of the Interior. Presented before the Society of Economic Geologists, Washington Meeting, Dec. 29, 1937.

that can be used to augment the flow of the Potomac during the summer months both for industrial uses and for sewage dilution at Cumberland. A dam 175 feet high and 1,050 feet long will provide the necessary storage capacity. The exploration and

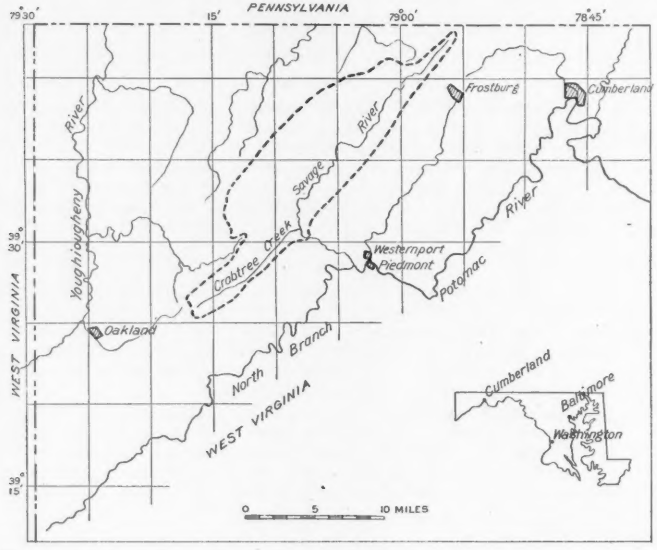


FIG. 1. Index map, showing watershed of Savage River above the proposed dam site.

design of the project were turned over to the Corps of Engineers under an allotment of funds from the Works Progress Administration. In December 1936, the Geological Survey was asked to provide a geologist to report on the geology and to advise on the choice of site. The assignment proved to be typical of the modern trend in the use of geology by engineers, in that the geologist had a voice not only in the original choice of site but was consulted throughout the period of exploration and design. Even though few geologic principles were involved, a composite history of the geologic investigations seems worthy of publication

as an example of the applicability of geology to engineering work. Certain features of the investigations, notably the results of pressure tests and the search for available construction materials, in which the geologist acted in a minor advisory capacity, are not considered here. The total cost of the geologic work was less than $1,000, an almost insignificant sum compared to the $100,000 exploration and design program, or the estimated $2,500,000 cost of the dam.

It is a pleasure to acknowledge my indebtedness to all of the engineers associated with the project for their many courtesies and for the fine spirit of cordial cooperation that existed throughout the assignment. In particular, I wish to thank Maj. W. D. Luplow, district engineer, E. F. Tippetts, designing engineer, W. P. Creager, consulting engineer, and F. L. Meara, soils engineer. Grateful acknowledgment is also due my colleagues of the Geological Survey, particularly G. F. Loughlin, Chief Geologist, for many suggestions and constructive criticisms.

GEOLOGY AND PHYSIOGRAPHY.[2]

The entire watershed of the Savage River lies in the Allegheny Plateau of the Appalachian Mountains. All the rocks of the Allegheny Plateau are of Paleozoic age and are relatively unmetamorphosed. Even though involved in broad folds the strata are relatively horizontal and are disturbed by a few small faults. The Savage River watershed is bounded on the northwest by Meadow Mountain and on the southeast by Big Savage and Backbone Mountains. These mountains, which rise to altitudes of 2,600 to 3,000 feet, are held up by the resistant sandstone and conglomerate of the Pottsville formation. Between the mountains there is a belt of Devonian and Carboniferous rocks that are softer than the Pottsville beds and so have been eroded to form the Savage River Valley.

The Savage River flows along the eastern part of this belt of

[2] Abbe, Cleveland, Jr.: The physiography of Allegany County. Md. Geol. Surv., Allegany County, pp. 27–56, 1900; The physiography of Garrett County. Md. Geol. Surv., Garrett County, pp. 27–54, 1902. Martin, G .C.: The geology of Garrett County. Md. Geol. Surv., Garrett County, pp. 55–182, 1902.

soft rocks in a deeply incised, somewhat meandering valley until near the mouth of Crabtree Creek, where it turns abruptly east in a deep gap through the ridge formed by Big Savage and Back bone Mountains. The course through the gap is not determined by an discernible structural weakness and it cuts through many beds that differ greatly in hardness and resistance to erosion. Its major features are obviously not controlled by the type of rock. Thus the present course of the river must be an inheritance from a much older topography or is due to a mere accident of erosion.

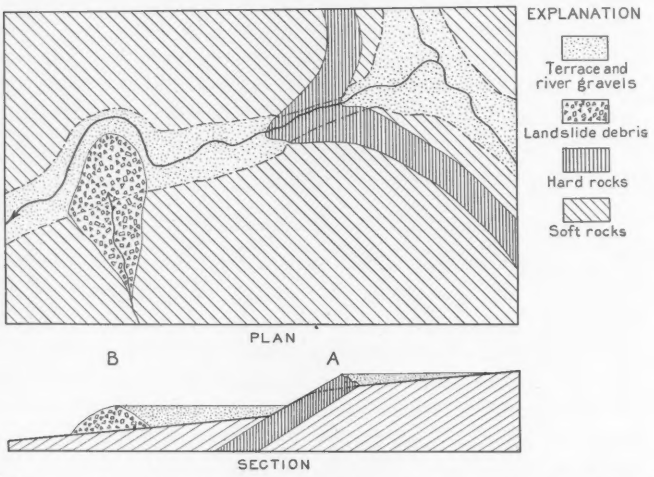


FIG. 2. Hypothetical plan and section showing mode of origin of river terraces. At *A*, a bed of durable sandstone caused abrupt narrowing of the valley; at *B*, landslide debris dammed the stream temporarily.

The stream is still cutting downward actively but during comparatively recent geological times it has been delayed in this action many times. In places hard beds of sandstone have retarded the downcutting; elsewhere landslides have dammed the stream temporarily. These pauses have led to the development of some of the meanders and to the dumping of boulders, gravel, and sand

on the valley floor. The gravels have since been cut through and their remnants now stand as flat-topped terraces along the river banks. Fig. 2 illustrates the two chief modes of origin of the terraces. Beds of sandstone erode more slowly than do the interbedded shales and hence stand out above the general level of the valley floor. Small deposits of gravel are commonly built

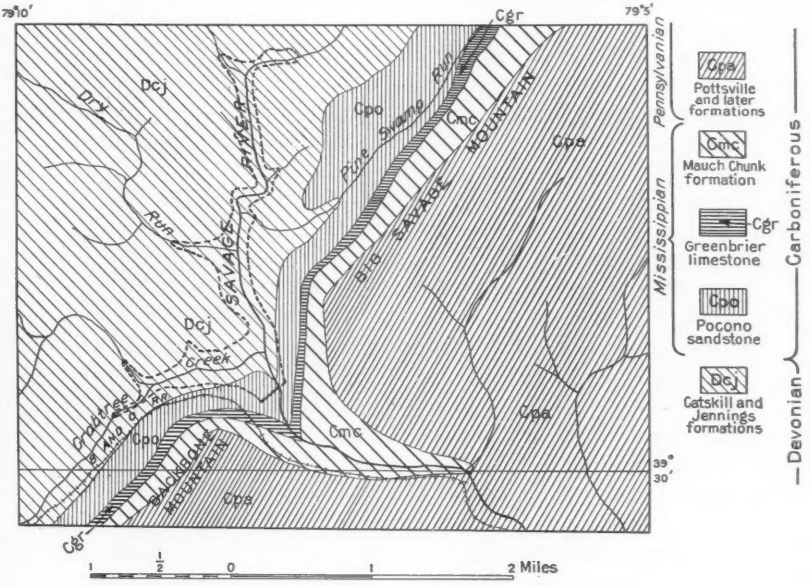


FIG. 3. Areal geology in vicinity of Savage River dam site, showing approximate flow line of reservoir. Adapted from Grantsville-Accident folio.

up behind such barriers, but the chief influence of the hard beds is to cause an abrupt narrowing of the valley, behind which deposits of alluvium are formed in the comparatively sluggish waters. One large deposit of gravel, which has since been cut through to form terraces at several levels, occurs at the mouth of Crabtree (Fig. 3).

Many remnants of terraces occur along the banks of the Savage River throughout the gorge between Big Savage and Backbone Mountains. Every terrace examined was found to terminate at its downstream end against the remains of a pile of debris that had been dumped into the main valley from some tributary valley or ravine. The debris is believed to represent landslides, avalanches, or torrential fans, since the products of slower erosion processes would probably have been carried away by the stream almost as fast as they accumulated. It seems clear that each of these piles of debris acted as a dam behind which gravels and sand were deposited by the river. In time the stream cut through the barriers or cut new channels around them, drained the temporary lakes that had been formed, and eventually cut a channel through the layers of gravel. Terraces along one or both banks of the stream resulted from this process.

Geology of Reservoir and Dam Site.—The general geology of the reservoir area and the dam site is simple. A series of beds of well-consolidated sandstone, shale, and limestone is well exposed along the valley of the Savage River. The general geology is described and mapped in the Accident-Grantsville folio [8] from which Fig. 3 has been adapted. The table below contains brief descriptions of the formations that occur in the vicinity of the site. Exposures are good in general and it was possible to measure a nearly complete section of the Greenbrier and Pocono beds that subsequently checked closely with the section developed by drilling.

The average strike of the beds is N. 45° E., and in no place near the dam site was a strike measured that deviated more than 5 degrees from this average. Local dips range from 6 to 25 degrees, with a general average of 13 degrees to the southeast, or downstream. There are no large faults but a few small faults with displacements of 1 to 2 feet were found. Fractures accompanied by little or no displacement are common. Most of them are nearly vertical, and they fall into two systems, which trend N. 30°–40° E. and N. 40°–60° W. Nearly all those seen

[8] Martin, G. C.: Geol. Survey Geol. Atlas, Accident-Grantsville folio (no. 160), 1908.

GEOLOGIC FORMATIONS IN VICINITY OF SAVAGE RIVER DAM AND RESERVOIR SITE.

	Formation top	Approximate thickness, (feet)	Character	Distribution
Pennsylvanian	Pottsville formation	325–375	Massive sandstone, conglomerate; some shale, coal, fire clay	Crops on top and side of Big Savage and Backbone Mts. Reaches level of Savage River, 1½ miles below dam site.
Mississippian	Mauch Chunk shale	650	Soft red and green shale, with some sandstone near base	Above dam and reservoir area. Band of outcrop 600–2,000 feet wide near crests of Big Savage and Backbone Mts. Forms bed of Savage River for long distance downstream from dam site.
	Greenbrier limestone	225	Limestone, sandy limestone, limy sandstone, and red and green shale	Foundation rock for spillway and upper part of abutments. Band of outcrop 200–1,000 feet wide between Mauch Chunk and Pocono on N.W. slopes of Big Savage and Backbone Mts. Forms bed of Savage River just below dam site.
	Pocono sandstone	450	Thin-bedded sandstone, with interbedded sandy shale	Foundation under dam and lower part of abutments. Band of outcrop 800–3,000 feet wide on N.W. slopes of Big Savage and Backbone Mts. Underlies valley floor and forms valley walls at most favorable dam site.
Devonian	Catskill formation	1,800–2,200	Soft red and green shale and sandstone	Occupies all of reservoir site except for small area near dam site.

are short and discontinuous. Most of them are open and hence subject to leakage only close to the present surface, and nearly all are tight in unweathered rock.

Water Table Conditions.—The effective water table is at or close to the present surface in the vicinity of the reservoir. Springs issue from the rocks at many places along the sides of the valley. Many of them are high above the flow line of the proposed reservoir. Most of these springs are probably con-

nected with local perched water tables where permeable beds are underlain by impermeable beds, but the general water table is probably nowhere far below the surface.

This situation means that when water is raised to higher levels by means of the dam it will rest against rocks that are either impermeable or are already saturated with water; therefore, losses by seepage into the reservoir walls should be small, even when the reservoir is first filled. Similarly, any water that enters the rocks that form the abutments would probably flow along the bedding planes and fractures, which would act as more or less confined passageways leading down steeply to the river. Since these openings are filled with water at and below the river level, and since a grout curtain will be provided along the center line of the dam, there will be no chance for circulation and consequently losses through the foundations or lower portions of the abutments of the dam should be small.

PRELIMINARY CHOICE OF DAM SITE.

Sites on the lower part of the river, where Pottsville sandstone or Mauch Chunk shale would form the foundations were eliminated largely because the stream grade is so steep that a much higher and more expensive dam would have been necessary to provide the desired storage and because it would have been necessary to relocate the Baltimore & Ohio Railway tracks. The choice of site was therefore limited to a three-quarter mile stretch near the upper end of the gorge. Three groups of rocks of very different character are exposed along this stretch (Fig. 3.). The Greenbrier limestone was regarded unfavorably as a foundation rock for two reasons. The shale that makes up about half of the Greenbrier section is relatively soft and it was thought possible that it would be weak and slippery when saturated with water. At least one extensive cave was known to exist in the limestone near the base of the Greenbrier beds. This cave is high above the flow line of the proposed reservoir but it was feared that other caves might exist at lower levels. The limestone is strongly jointed and solution along planes has caused formation of open fissures in many places.

The Pocono sandstone was regarded most favorably as a foundation rock. The beds of sandstone that make up two-thirds of the group are fine- to medium-grained and thin-bedded; most of them are somewhat shaly. It was believed that they were strong enough to support any type of structure of the proposed size and that they were relatively water-tight. The interbedded shaly layers, most of which are somewhat sandy, would, of course, be impermeable and they appeared to be harder and more durable than other shales both higher and lower in the stratigraphic section.

The Catskill beds were so soft and shaly that they seemed likely to soften in water and to slake on exposure, with consequent danger of slipping under load; furthermore, topographic conditions in the area underlain by Catskill beds were not as attractive as at places farther downstream. The possibility of finding a site on the Catskill formation, therefore, was not investigated further.

In my original report it was stated, "The rocks that make up Group 2 (Pocono sandstone) are unquestionably the most favorable as foundations for the dam. If an earth dam is built, there is no reason why parts of the downstream or upstream slopes should not rest on the rocks of Groups 1 or 3 (Greenbrier or Catskill), respectively, but the main part of the structure should rest on the rocks of Group 2. . . . The topography is also most favorable between these points, as the valley is narrower there than elsewhere." It was mutually understood that the superiority of the Pocono beds as foundation rocks was merely a question of degree and that it was entirely feasible to build a successful dam on any of the rocks exposed along the Savage River.

Much of the valley floor is covered by deposits of sand, gravel, and boulders, which form terraces that rise from 2 to 20 feet above the river level. It was pointed out that the exact profile of the underlying bedrock floor could be determined only by drilling or excavation. Since bedrock crops out in the stream in many places and since there is no evidence that the river channel was ever much deeper than now, it was believed that bedrock

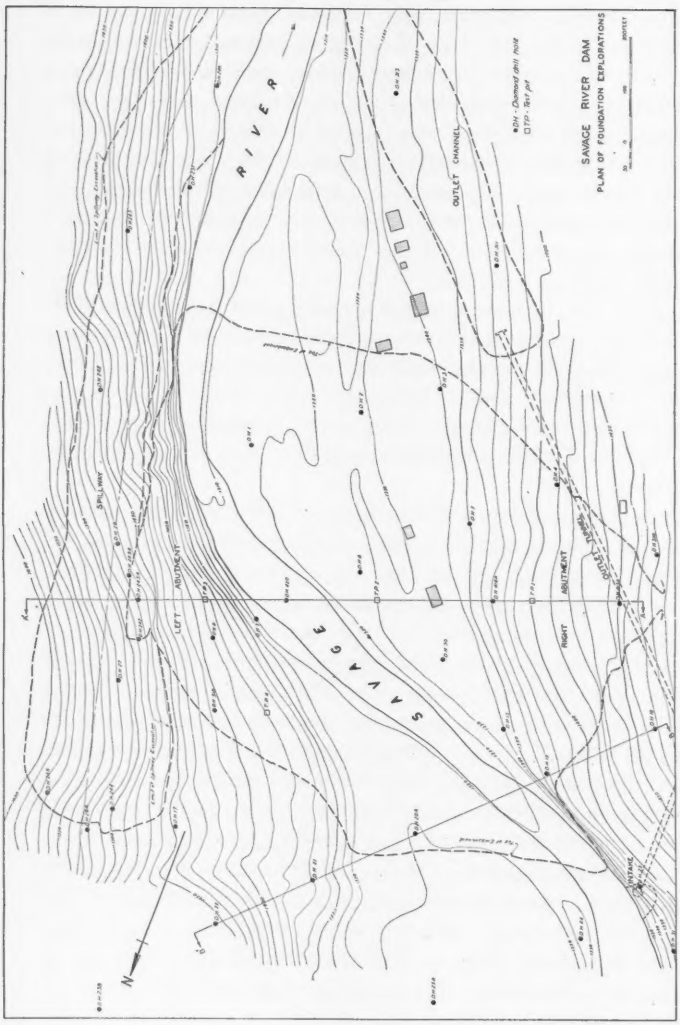


FIG. 4. Plan of foundation explorations at Savage River dam site. Adapted from map prepared by U. S. Engineer Office, Washington, D. C.

would be found at about the level of the present stream bed and that it should be fresh or only slightly weathered beneath the terrace gravels. The origin of the terraces as outlined above and shown in Fig. 2 led to the suggestion that the deepest part of the valley floor might well lie somewhere beneath the terraces rather than under the present stream channel. These predictions were later verified by drilling (Fig. 5). It was also pointed out that the terrace gravels should be explored in order to determine their characteristics and permeability. They might prove to be so weak and permeable that their removal before construction of the dam would be advisable. On the other hand, it was thought possible that it would only be necessary to construct a cut-off wall down to bedrock.

FIG. 4. Plan of foundation explorations at Savage River dam site. Adapted from map prepared by U. S. Engineer Office, Washington, D. C.

EXPLORATION AND FINAL CHOICE OF SITE.

Three holes were drilled in the valley floor several hundred feet downstream from the preliminary site in order to check the section measured on surface exposures and to provide more precise data on the characteristics of the various beds. The holes were located near the place where the top of the Pocono sandstone dips underground and were deep enough to yield a nearly complete section of the Pocono beds. These preliminary holes showed the following facts:

(1) The zone of strongly weathered, unsound rock is comparatively shallow; slight weathering extended to depths of 43 to 66 feet. The rocks classed as "strongly weathered" are soft and crumbly and are obviously unsound. Those classed as "slightly weathered" included all those rocks that show the least tendency toward bleaching or cracking, or which slake on exposure. Most of them would normally be classed as sound rock and in their natural condition they should be essentially water-tight and strong enough to support the proposed structure. This distinction between strongly and slightly weathered rocks was made throughout the subsequent investigations and reports on drill cores.

(2) The rocks are saturated with water below the river level.

(3) Below the strongly weathered zone all bedding planes are tight, and fractures, though present, tend to be tight and discontinuous.

(4) Both shales and sandstones are hard and strong.

(5) A few beds that are characterized by the presence of small lenses and balls of soft clay and shale in a sandstone matrix or by thinly interbedded shale and sandstone appear to be weak and give poor core recovery. These beds were known to be thin and were thought to be lenticular in large part, however.

Several sites were studied with regard to suitability of foundations for the embankment, for possible location of the spillway and outlet structures, and for comparative costs. All the sites were located so that as far as possible, the foundation of the embankment would be on the Pocono sandstone and so that the abutments would consist of Pocono sandstone or Greenbrier limestone. In all, 44 diamond drill holes and 4 test pits were put down.

The results of borings at the first site selected (Fig. 4 section B–B′) made it necessary to abandon it. The rocks themselves, shown in the section, Fig. 5, were excellent and the zone of strong weathering was comparatively shallow. The borings showed, however, that the shape of bedrock beneath an alluvial cone on the left (northeast) abutment was much different from that suggested by surface indications. The cone and the lower part of the ravine above it are indicated by contours in the upper left-hand corner of Fig. 4. There, the overburden was shown to be 56 feet deep under the center line of the dam and the walls of the lower part of the valley were shown to be nearly vertical. The core section of the dam would rest on a 56-foot depth of overburden, whereas the toes would rest on a much smaller depth, a condition that might cause unequal foundation consolidation and resultant embankment stresses. Spillway construction would necessitate excessive concrete work and would probably create a dangerous slide condition where the spillway crossed the debris-choked ravine above the alluvial cone. Since it was not feasible to locate a spillway on the right (southwest) abutment because

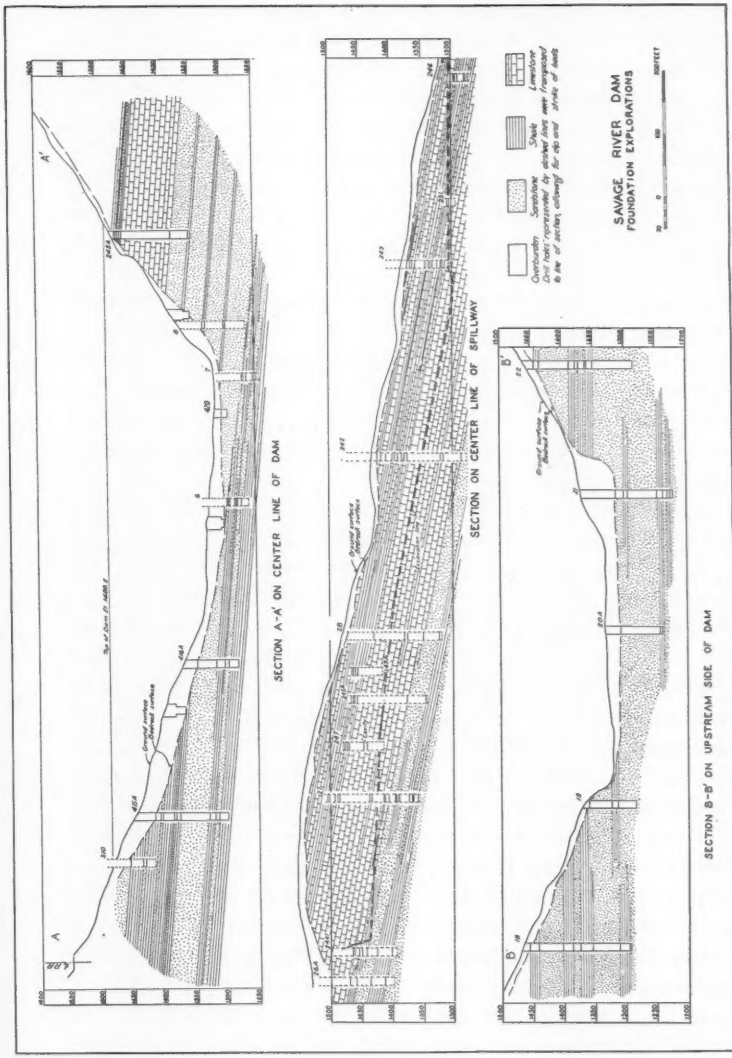

SECTION A-A' ON CENTER LINE OF DAM
SECTION ON CENTER LINE OF SPILLWAY
SECTION B-B' ON UPSTREAM SIDE OF DAM
SAVAGE RIVER DAM
FOUNDATION EXPLORATIONS
Overburden
Sandstone
Shale
Limestone
Drill holes represented by dotted lines were transposed to line of section, although for the most part strike of beds
SCALE OF FEET

FIG. 5. Sections showing results of foundation explorations at Savage River dam site. From maps prepared by U. S. Engineer Office, Washington, D. C.

of danger to the Baltimore & Ohio Railway tracks, it was not possible to use any of the sites near the original site. At all of them it would be necessary to rest the dam against the alluvial cone or to cross it with the spillway.

The site that was finally selected lies along the line of section A–A′ (Figs. 4 and 5). This selection was made so that the embankment would rest on Pocono sandstone, whereas the spillway would be cut in Greenbrier limestone below the debris-filled ravine. The final site was thus a compromise based almost entirely on geological considerations.

Caves in Limestone.—As shown in Fig. 5, the foundation rocks for the concrete-lined spillway, the 320-foot spillway intake wier, and the upper part of the left abutment are of Greenbrier limestone. The spillway itself is to be excavated from a thick bed of massive limestone. The decision to locate the spillway structures on limestone brought up several interesting problems in applied geology.

Aside from a small cave exposed at the surface and an 8-inch cavity found in one of the early drill holes, no caves of consequence were discovered during the drilling of 11 holes that entered or penetrated the limestone horizon. Even the geologist had become lulled to the belief that his fears of extensive caves were groundless. Near the close of the drilling campaign, however, one of the drill holes penetrated a 3- to 5-foot cave in the limestone. Since it underlay the proposed spillway intake, on the upstream side of the dam, the discovery called for careful consideration with respect to strength of the spillway foundations, possibility of serious leakage, and possible presence of other but undiscovered cavities in the limestone. The hole in which the cave was reported had encountered cavernous conditions from the surface down to the main cave at a depth of 40 feet. Six small cavities, all but one of which appeared merely as notches in the side of core, were noted in the log. The driller reported that the main cave was not actually open and was filled with soft rock, probably travertine, that could not be recovered in the core. The driller further reported that he felt almost certain that the " cave "

was actually a crevice and that the core barrel was probably touching the hard rock walls of the crevice in most places. The shape and extent of the opening are still unknown. The numerous small cavities throughout the core, however, the driller's report, the observed shape of other openings along the exposed ledge of limestone, and analogy with a large cave on the right side of the river all indicate the opening to be a narrow, nearly vertical fissure. Its vertical extent is possibly limited only by the top and bottom of the limestone series and it may extend a considerable distance from the surface back into the hillside. In all probability the fissure follows joint planes and has a pronounced zig-zag course in both the vertical and horizontal planes. There are certainly no very large flat openings, parallel to the bedding planes, because three nearby drill holes penetrated the same beds but encountered only one very small cavity.

Without a large number of closely-spaced drill holes or the sinking of a large shaft it seems impossible to discover the exact shape or extent of the open fissure. Other similar fissures may have escaped detection during the drilling program, but they do not offer any serious obstacle to the construction of an earth dam or of a concrete-lined spillway. The rock abutments, though naturally weakened by such fissures, are certainly much stronger and able to withstand loading than if the caves were flat and of large horizontal extent. An extensive grouting program along the left abutment has been included in the design. The soil and alluvial cover over the limestone will be removed to locate any surface openings, which will be filled with concrete or grouted. The limestone beds are at higher elevations on the right or southwest abutment and will not require extensive grouting.

Use of Shale in Embankment.—Limy shale and limy sandstone will form a considerable percentage of the spillway excavation. For purposes of economy, the engineers desired to use this material in construction of the dam, provided they could feel certain that they would prove stable and would not break down later to soft clay that would endanger the structure. A series of representative samples of shaly material were subjected to cycles of

alternate wetting and drying. Some of the samples showed a tendency to crumble after a few cycles, but none of them broke down to clay or mud. An effort was then made to discover the behavior of similar shales when placed in embankments over a long period of time. The following requisites guided the search:

(1) Embankment must be as old as possible.

(2) Approximate age of embankment should be known.

(3) Embankment should be made up in large part of free-draining lump material.

(4) Shale should be derived from the same geologic horizon as shales at the Savage River site if possible and in any event must be comparable in physical characteristics.

This work resulted in finding three embankments that fulfilled most of the requirements. Samples of shale and limy shale were collected from the downstream face of the Deep Creek dam, 13 miles west of the Savage River site, from the 25-year old spoil bank at the limestone quarry of the Alpha Portland Cement Co.'s plant at Manheim, W. Va., and from a recent highway embankment near Rowlesburg, W. Va. The shale in all three of these embankments showed the same characteristics. When actually exposed to the weather the shale crumbles rapidly. When protected from the weather by a few inches of soil or by the crumbled shale itself, however, the lumps of shale are stable. The evidence indicated, therefore, that it would be safe to use shale in the Savage River dam, provided its use was confined to the well-drained rock fill portion of the downstream slope and that it was protected from weathering and erosion by a layer of riprap or of grass covered soil. The engineers did not concur in these conclusions until after a confirmatory series of freezing and thawing tests on representative samples had been made.

The more resistant limy shales, as determined by freezing and thawing tests during construction, mixed with the thin beds of sandstone and with the limestone, will be placed in the upper portion of the downstream dam embankment, well above the saturation line and well above the elevation of highest tailwater. Weathered shale and the poorer classes of shale and mudstone,

as determined by freezing and thawing tests during excavation, will be wasted in nearby spoil areas. The decision that it will be safe to use most of the shale in the dam embankment, which was based in part on the findings and opinions of the geologist, will result in savings approximately equal to the cost of the exploration and design of the structure. The problem also gives point to the need for a classification and definition of rock types that will be of use to engineers. To many engineers the word *shale* connotes a type of rock with which they have had many unfortunate experiences in the past. To the geologist the word includes a variety of rocks that possess several different physical characteristics. It is difficult to formulate a classification that will properly take account of all the gradations that exist in the sedimentary rocks and that will give some idea of the relative durability of the different classes. At present it seems necessary to make liberal use of modifying adjectives when describing a given rock, to retain and exhibit representative samples of the rock types described, and to show examples of gradation from one type to another to the engineers, either in drill cores or in natural exposures. Decisions as to probable durability must be based on the behavior of the rock in natural and artificial exposures and on physical tests such as cycles of alternate wetting and drying or of freezing and thawing.

CONCLUSION.

The foregoing description of the Savage River site might lead to the conclusion that in view of the demonstrated excellence of the foundation rocks, the regularity of the beds, and the simplicity of the geologic structure that the site was over-geologized and over-drilled. It must be pointed out, however, that the true character and inherent danger of the alluvial cone described above did not become apparent until the drilling was well under way. Furthermore, although the possibility of caves in the limestone had been stressed from the first, the actual existence of caves was not proved until nearly the close of the drilling campaign. The discovery, like the decision to use shale in the em-

bankment, led to important changes in design. The results of this investigation give added justification to the belief held by most present-day engineers, that the foundations of every dam site, no matter how well exposed, should be thoroughly explored by means of drill holes, test pits or trenches, and that the value of geological studies commonly far exceeds the cost thereof.

U. S. Geological Survey,
Washington, D. C.,
Jan. 26, 1938.

EXPERIMENTS BEARING ON THE RELATION OF PYRRHOTITE TO OTHER SULPHIDES.*

R. L. HEWITT.

ABSTRACT.

The problem of the existence of solid solutions between pyrrhotite and pentlandite, chalcopyrite, galena, and sphalerite was investigated by heat treatment. Pentlandite was put into solid solution in pyrrhotite above 425° C. Upon slow cooling from 800° C. it unmixed and oriented itself around pyrrhotite grains; under high magnifications was observed to contain oriented laths of a mineral tentatively called pyrrhotite. The formation of these laths may be caused by the unmixing of a solid solution of pyrrhotite in pentlandite or by the breakdown of pentlandite expelling pyrrhotite.

Pyrrhotite and chalcopyrite form two solid solutions. Pyrrhotite will dissolve in chalcopyrite above 300° C. The two minerals then react to form chalcopyrrhotite which makes a very fine intergrowth with chalcopyrite in an aureole about pyrrhotite masses in the samples treated. Above 600° C., chalcopyrite will dissolve in pyrrhotite and, upon unmixing, forms oriented laths in the pyrrhotite.

The eutectic intergrowth formed by pyrrhotite and galena has a composition of approximately 71 per cent galena and 29 per cent pyrrhotite by weight. The eutectic temperature lies between 765° and 775° C. The variation between these figures and those for the artificial PbS-FeS system may be caused by impurities.

Crystallographic intergrowths between pyrrhotite and sphalerite were never produced, although such intergrowths have been described and observed in many ores and it is assumed that hydrothermal conditions lower the temperature of formation for such intergrowths considerably below that required in a dry melt.

Small crystals of pyrrhotite, chalcopyrite, and galena were formed and the evidence indicates that they were produced by volatilization and subsequent deposition.

INTRODUCTION.

PYRRHOTITE has long been considered an important diagnostic mineral in the classification of ore deposits. It is normally a high temperature mineral and occurs associated with other high

* Presented before the Society of Economic Geologists, Washington Meeting, Dec. 29, 1937.

temperature sulphides and common rock-forming silicates. It may be accompanied, however, by sulphides that have a wide temperature range. In spite of its common occurrence the relation of pyrrhotite to other sulphides has been somewhat neglected. A recent paper by Schwartz [1] deals with the paragenesis of pyrrhotite and emphasizes its presence in ore deposits throughout the world. Many papers have been written describing the relationship of pyrrhotite to pentlandite but little experimental evidence has been given to supplement and corroborate the microscopical observations and conclusions. It was thought, therefore, that experimental research into the relation of pyrrhotite to several common sulphides would be profitable.

The experiments were designed to investigate the characteristics of any crystallographic intergrowths, produced by unmixing of pyrrhotite and other sulphides. Pentlandite, chalcopyrite, galena and sphalerite were used in the experiments in combination with pyrrhotite.

Materials and Methods.—The material for this work was obtained from various suites of ores in the Geology Department of the University of Minnesota. The specimens selected were examined closely both in the hand specimen and in the polished surface in order to reduce to a minimum the presence of sulphides other than those required. The material chosen for the pyrrhotite-pentlandite series came from Sudbury, Ontario; the pyrrhotite-chalcopyrite set came from Parry Sound, Ontario; and the pyrrhotite-galena specimens came from the Sullivan Mine, Kimberly, B. C. Ore from the Reno Mine, B. C., provided the pyrrhotite-sphalerite material.

The procedure followed in this investigation may be outlined thus: A chip of ore, usually not more than ½ inch square and of varying thicknesses, was polished and examined under the metallographic microscope. It was then sealed, in accordance with the temperature of the experiment, in either a pyrex or

[1] Schwartz, G. M.: The paragenesis of pyrrhotite. ECON. GEOL., vol. 32, pp. 31–55, 1937.

fused silica glass bomb. These bombs were not evacuated as oxidation was negligible if care was taken to see that the specimen was thoroughly dry. The bomb was placed in a horizontal, electrical tube furnace and the furnace ends packed with asbestos. The temperature was determined by a potentiometer attached to a calibrated chromel-alumel thermocouple, which was thrust well into the furnace before the ends were packed. The bombs were heated from 6½ to 169 hours and then quenched by cooling out of doors. The bombs were broken open and any tarnish present on the sample was removed by polishing. In some cases it was desirable to reheat a part of the sample and have it cool slowly in order to facilitate unmixing. This was accomplished by raising the temperature of the furnace to that used in the previous experiment and then slowly lowering it. The length of time required for slow cooling depended upon the experiment. The treated samples were examined under both high and low magnifications and any changes noted. Photographs were taken of any new intergrowths or textures.

Acknowledgments.—The writer wishes to take this opportunity to express his sincere gratitude to Drs. G. M. Schwartz and F. F. Grout for their helpful suggestions and impartial criticism during the preparation of this paper.

PYRRHOTITE-PENTLANDITE RELATIONSHIPS.

Pentlandite normally occurs as equigranular grains with pyrrhotite or as smaller grains and stringer-like masses along the borders of pyrrhotite grains. Pentlandite may also appear as oriented blades, lenses, or flakes within pyrrhotite crystals.

Many of the early writers considered that all the pentlandite was later than pyrrhotite. This view is held by Campbell and Knight [2] who state that the pyrrhotite was fractured before the pentlandite was deposited. Wandke and Hoffman,[3] Lindgren

[2] Campbell and Knight: On the microstructure of nickeliferous pyrrhotite. ECON. GEOL., vol. 2, pp. 350–366, 1907.

[3] Wandke and Hoffman: A study of the Sudbury ore deposits. ECON. GEOL., vol. 19, pp. 169–204, 1924.

and Davy,[4] Kerr,[5] and Buddington [6] also published similar ideas.

In opposition to the above conceptions several writers believe that pentlandite is formed entirely by the unmixing of a solid solution. Schneiderhöhn [7] and Van der Veen [8] are the chief supporters of this theory. Van der Veen states that pyrrhotite and pentlandite unmix at a high temperature, where diffusion is rapid and results, in general, in complete separation of the two minerals.

Ehrenberg [9] describes several oriented intergrowths of pentlandite in pyrrhotite from Miggiandone and Sudbury ores.

More recent papers on the subject tend toward a compromise of ideas. Scholtz,[10] in describing the nickeliferous ore deposits of East Griqualand, South Africa, states:

> A condition of limited miscibility set in at the freezing point of pyrrhotite which induced the separation of the major portion in the form of pentlandite more or less contemporaneously with the crystallization of the pyrrhotite which held a certain amount of pentlandite molecule in solid solution. Shortly after consolidation a nickel-bearing phase segregated from the pyrrhotite and concentrated principally about the grain boundaries, inclusions and cracks of early origin. . . . A consideration of the distribution of the pyrrhotite and pentlandite in the different varieties of the ore leads one to the conclusion that, although both minerals appear to have separated more or less simultaneously the latter continued to crystallize after the former had been completely precipitated.

Newhouse [11] discusses the subject and bases his conclusions on a series of experiments with synthetic pyrrhotite and pentlandite. He states that narrow, blade-like or lens-like masses of pentlandite within pyrrhotite crystals, similar to those observed in pyrrhotite-

[4] Lindgren and Davy: Nickel ores of the Key West mine, Nevada. ECON. GEOL., vol. 19, pp. 309–319, 1924.

[5] Kerr, P. F.: A magmatic sulphide ore from Chicagof Island, Alaska. ECON. GEOL., vol. 19, pp. 369–376, 1924.

[6] Buddington, A. F.: Alaskan nickel minerals. ECON. GEOL., vol. 19, pp. 521–541, 1924.

[7] Schneiderhöhn, H.: Anleitung zur mikroscop. Bestimmung von Erzen, pp. 178, 1922.

[8] Van der Veen, R. W.: Mineragraphy and ore deposition. vol. 1, pp. 41, 1925.

[9] Ehrenberg, H.: Orientierte Verwachsungen von Magnetkies und Pentlandit. Zeit. fur Kryst., vol. 82, pp. 309–315, 1932.

[10] Scholtz, D. L.: The magmatic nickeliferous ore deposits of East Griqualand and Pondoland. Univ. of Pretoria, part 1, 1933.

[11] Newhouse, W. H.: The equilibrium diagram of pyrrhotite and pentlandite and their relations in natural occurrences. ECON. GEOL., vol. 22, pp. 288–299, 1927.

pentlandite ores, may be formed by the break-down of a solid solution of pyrrhotite and pentlandite. He also states that pentlandite may segregate in the liquid phase.

The conclusions of Newhouse and Scholtz appear to be closer to the truth than any of the other explanations as they account for the oriented intergrowths and also for some occurrences of pentlandite in pyrite, such as are described by Walker [12] and by Lindgren and Davy.[13]

Description of the Experiments.

Material.—The Sudbury specimens selected were massive nickeliferous pyrrhotite free from gangue. Pyrrhotite made up 90 per cent of the ore, and pentlandite, chalcopyrite and magnetite comprised the remainder. Only pyrrhotite and pentlandite were present in most of the specimens, but magnetite and chalcopyrite made up ½ per cent of a few. The pyrrhotite was in coarse, interlocking, anhedral grains haphazardly oriented. Most of the pentlandite was in small, granular stringers around pyrrhotite grain boundaries, or as rounded masses included in pyrrhotite grains. Slender blades of pentlandite were also observed in a similar location.

The magnetite occurred as small, corroded remnants in all of the sulphides and is undoubtedly of an earlier origin than any of them. The chalcopyrite was in small irregular masses that occupied fractures in the other sulphides or gangue-sulphide contacts.

The experiments and their controlling conditions, and a summary of the observations on each are given in Table I, which also shows the order in which the experiments were carried out and the relation of one experiment to another.

Observations.—Experiments Nos. 1, 2, and 3 were of an exploratory nature with regard to the length of time required for heating. These were run for 96, 72 and 46½ hours respectively

[12] Walker, T. L.: Certain mineral occurrences in the Worthington mine, Sudbury, Ontario, and their significance.

[13] Lindgren and Davy: *Op. cit.*

TABLE I.

EXPERIMENTS ON PYRRHOTITE AND PENTLANDITE.

Exp.	Temp.	Time	Time of Cooling	Observations
1.	600° C.	96 hrs.	5 min.	Pentlandite dissolved in pyrrhotite; hexagonal pyrr. crystals grew on surface.
2.	600° C.	72 hrs.	5 min.	Pent. dissolved in pyrr.
3.	600° C.	46½ hrs.	5 min.	Pent. and chalcopyrite dissolved in pyrr.; pyrr. crystals grew on surface.
4.	600° C.	1 hr.	24 hrs.	Part of the product of No. 3 was used.
5.	400° C.	47 hrs.	5 min.	No change.
6.	500° C.	48 hrs.	5 min.	Pent. and chal. dissolved in pyrr.
7.	500° C.	48 hrs.	5 min.	Pent. and chal. dissolved in pyrr.
8.	500° C.	1 hr.	15 hrs.	The product of No. 7 was used; no change.
9.	500° C.	48 hrs.	5 min.	Pent. dissolved in pyrr.
10.	900° C.	48 hrs.	5 min.	Pent. dissolved in pyrr.
11.	800° C.	12 hrs.	60 hrs.	A part of the product of No. 10 was used. Pent. unmixed from pyrr. in rounded masses. Pyrr. was seen as small oriented laths in unmixed pent.
12.	800° C.	42 hrs.	5 min.	Pent. dissolved in pyrr.
13.	800° C.	12 hrs.	10 hrs.	A part of product of No. 12 was used. Pent. unmixed from pyrr. around the grains of pyrr. Small laths of pyrr. in unmixed pent.
14.	800° C.	48 hrs.	30 hrs.	Pent. dissolved in pyrr.
15.	460° C.	48 hrs.	5 min.	Pent. dissolved in pyrr. but chal. did not.
16.	425° C.	47 hrs.	5 min.	No change.
17.	400° C.	47 hrs.	5 min.	No change.
18.	500° C.	20 hrs.	6 hrs.	A part of the product of No. 15 was used; no change.
19.	460° C.	48 hrs.	5 min.	Pent. dissolved in pyrr.
20.	450° C.	20 hrs.	4 hrs.	A part of the product of No. 19 was used; no change.

and similar results obtained. The pentlandite recognized before heating could not be recognized at the close of the experiment. In some specimens remnants of large pentlandite masses remained after heat treatment. Well defined veinlets that previously contained isotropic pentlandite, after heating held anisotropic pyrrhotite (Fig. 2). As these observations were common to all three experiments, the time, 48 hours, was adopted for the later experiments.

The temperatures used differed in the various experiments and those for later experiments were determined by the observations of earlier. The highest temperature, 900° C., was reached in No. 10, while the lowest, 400° C., was used in No. 5. The high temperature followed by slow cooling, was needed to induce

the pentlandite to unmix from the pyrrhotite. The lower temperatures of Nos. 15 to 17, were for the purpose of finding the solution temperature of pentlandite in pyrrhotite.

Pentlandite unmixed from pyrrhotite in some experiments when cooling was slow. These results were obtained from specimens in which the pentlandite had been put into solution by heat treatment. They were then heated up to 800° C. and allowed to cool slowly. The pentlandite unmixed from the pyrrhotite in Nos. 11 and 13. In No. 11, pentlandite formed little rounded masses within the pyrrhotite grains. In No. 13, it was in rounded masses and stringer-like areas that segregated about the pyrrhotite grain boundaries (Figs. 3, 6). These new pentlandite areas do not occur in the same grains as did the pentlandite that went into solution. Most of the stringers are smaller than those of the original ore and enclose smaller pyrrhotite grains. Pentlandite occurs also as small, lath-like masses in pyrrhotite. These laths are, in some places, oriented symmetrically; in others they are in heterogeneous clusters (Fig. 4, 6). The pentlandite, unmixed from pyrrhotite, is not homogeneous under the oil-immersion lens, but has small rod-like masses oriented symmetrically throughout. These rods are anisotropic and slightly harder than the pentlandite. They are thought to be pyrrhotite (Fig. 7).

The samples that unmixed did so on slow cooling from 800° C. Several specimens in which the pentlandite had previously been put into solution in the pyrrhotite were heated to temperatures between 450° and 800° C. and cooled slowly. At these lower temperatures the material remained unchanged.

Hexagonal crystals of pyrrhotite were produced in experiments Nos. 1 and 3 and in several dealing with pyrrhotite and chalcopyrite (Fig. 5).

Interpretation of Results.

Application of Equilibrium Diagram to Pyrrhotite-Pentlandite Relationships.—The behavior of pyrrhotite and pentlandite in the foregoing experiments can best be understood by referring to an equilibrium diagram. Fig. 1 is the equilibrium diagram for pyrrhotite and pentlandite as given by Newhouse[14] and is the

[14] Newhouse, W. A.: *Op. cit.*

compilation of the results of a series of experiments using artificial pyrrhotite and pentlandite. The construction points of the diagram were established by cooling curves and lie along the

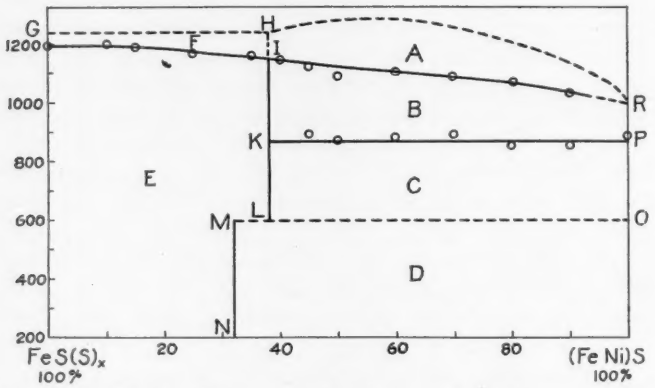


FIG. 1. Equilibrium Diagram of Pyrrhotite and Pentlandite. (After Newhouse.)

lines *G–R* and *K–P*, which are the loci of the freezing points of pyrrhotite, pentlandite, and mixtures. He states that pyrrhotite will take nickel into solid solution up to approximately 13 per cent. This is equivalent to approximately 40 per cent pentlandite, and is represented by the line *I–L–M–N* in Fig. 1. In the present investigation pentlandite, which occurred only as stringers around pyrrhotite grain boundaries, went entirely into solution. Large masses of pentlandite were only partially affected. This seems to be in agreement with Newhouse's results on the partial solubility of pentlandite in pyrrhotite although no accurate quantitative data were obtained.

The line *M–L* of the equilibrium diagram represents the temperature at which the pyrrhotite-pentlandite solid solution unmixes. Newhouse was unable to establish the temperature for this line definitely with the apparatus at his disposal. The writer's results show that pentlandite will dissolve in pyrrhotite at a minimum temperature between 425° and 450° C. No un-

mixing of the pentlandite, was seen, however, in specimens heated below 800° C. This may be due to the fact that at lower temperatures the specimens were extremely brittle and any evidence of unmixing may have been lost in polishing; or cooling may have been too rapid. It seems reasonable to suppose that pyrrhotite and pentlandite unmix at a temperature similar to or slightly lower than that at which they go into solution. If so, then the line *M–L* in Fig. 1 should be lowered to a position between 425° and 450° C. The fact that pentlandite was made to segregate around pyrrhotite grain boundaries is definite evidence of unmixing and proves that an exsolution temperature (*M–L* or some other) does exist in natural ores.

Fig. 7 shows a grain of pentlandite that unmixed from pyrrhotite. Within the pentlandite grain are small, symmetrically oriented, lath-like masses of pyrrhotite. The above relation of pyrrhotite and pentlandite, which was observed in experiments Nos. 11 and 13, needs explanation. There are at least two possible explanations of this behavior. First, it may be considered that the pentlandite grain was at one time a solid solution of pyrrhotite in pentlandite and that the pyrrhotite unmixed from the pentlandite on cooling. In Fig. 1, Field *A*, the lower boundary of which is the locus of freezing points for pyrrhotite (*I–R*), is a field in which pyrrhotite and pentlandite are said to exist as immiscible liquids. Field *B* contains solid pyrrhotite and liquid pentlandite and its lower boundary (*K–P*) is the locus of the freezing points of pentlandite. Fields *C* and *D* contain solid pyrrhotite and pentlandite. None of these fields contain a solid solution of pyrrhotite in pentlandite and if our assumption is correct that the pyrrhotite in the laths in Fig. 7 unmixed from the pentlandite, then Fig. 1 must be modified. In order to create a field of pentlandite solid solutions, Field *A* must contain liquids that are at least partially miscible or the field must not extend completely over to the 100 per cent pentlandite end of the diagram. The fact that the line *I–R* is slanting and not horizontal indicates that the composition of the pyrrhotite is changing and thus the liquids are at least partially miscible. This would allow for a

field of pentlandite solid solutions near the pentlandite end of the diagram from which the pyrrhotite might unmix under the proper conditions.

The second explanation is that pentlandite breaks down and expels pyrrhotite. Hayward [15] found that pentlandite, if held above its melting point for some time, breaks up into two components, ferrous sulphide, which probably contains an excess of sulphur, and a white substance that was called Ni_3S_2. This observation confirms the break-up of pentlandite but no mention was made as to its behavior at temperatures below the melting point. Further work is needed, therefore, before the relations of pyrrhotite and pentlandite are fully explained.

Textures.—Oriented intergrowths between pyrrhotite and pentlandite are relatively scarce and few have been observed. Van der Veen [16] gives an illustration of what appears to be an oriented intergrowth. Similar textures were observed in the ore studied in this work. Newhouse [17] describes intergrowths of oriented lenses of pentlandite within pyrrhotite crystals. Gruner [18] discusses the absence of oriented intergrowths in pyrrhotite-pentlandite mixtures by comparing the internal structure of the two minerals. He shows that if the sulphur atoms in the (111) plane of pentlandite are superimposed over the sulphur atoms in the (0001) plane of pyrrhotite, large holes in the pentlandite structure are apparent. He concludes that this rules out a sulphur contact plane. The metal atoms show excellent agreement but Gruner states that such a contact plane is impractical as the forces acting on some of the atoms on one side of the contact plane are much greater than those on the other side.

Ehrenberg,[19] in a more recent paper, gives some excellent illustrations of oriented intergrowths of pentlandite in pyrrhotite. The pentlandite in one of these intergrowths has a skeletal ap-

[15] Hayward, C. R.: The equilibrium diagram of the system Cu_2S-Ni_3S_2. A. I. M. E., Trans., pp. 141, 1914.

[16] Van der Veen: *Op. cit.*

[17] Newhouse, W. H.: *Op. cit.*

[18] Gruner, J. W.: Structural reasons for oriented intergrowths in some minerals. Amer. Min., vol. 14, pp. 227–237, 1929.

[19] Ehrenberg, H.: *Op. cit.*

pearance and has its arms oriented in three directions, 120 degrees apart. In another the pentlandite has an arborescent appearance. He states that the pentlandite has a three and six fold axis in intergrowths from Miggiandone and Sudbury respectively. He investigated the crystal structure of the two minerals and concluded that the three fold axis of pentlandite lies parallel to the *c*-axis of pyrrhotite and that the (110) plane of the former coincides with the (1010) plane of the latter. He used the metal planes as contact planes and states that the forces of the sulphur planes on either side of the contact plane are in equilibrium. No intergrowths similar to the above were produced in this work.

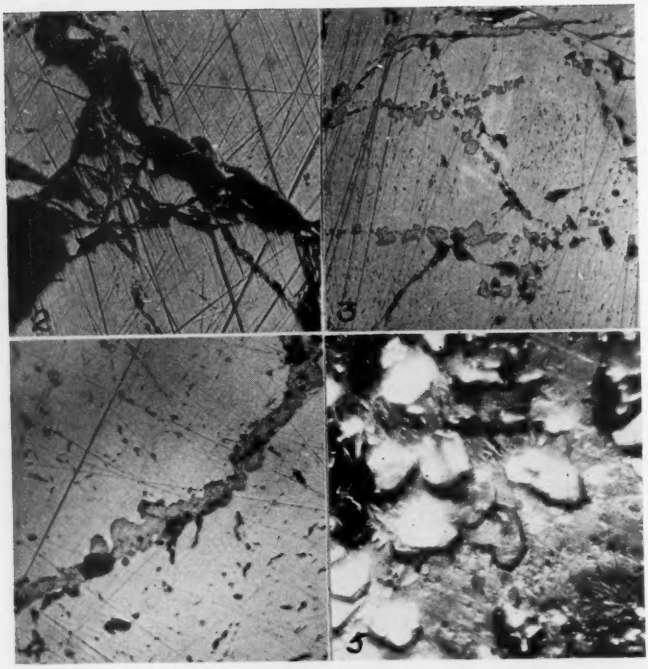
Natural oriented intergrowths of pyrrhotite in pentlandite are scarce and the writer's material did not show any. Ehrenberg [20] states that such an intergrowth was seen in ore from the Creighton Mine, Sudbury, Ontario. Fig. 7 shows an oriented intergrowth of pyrrhotite in pentlandite but it was produced by heat treatment. These intergrowths are of the lamellar or Widmanstatten type and seem to show the isometric characteristics of pentlandite.

Crystals.—Minute crystals developed over part of several specimens that were heated above 500° C. for 24 hours or longer. The crystals are euhedral where they had room to grow but some grew in crowded clusters with only a few faces on each. They occur more abundantly at one end of the specimen than the other. Examination under a binocular microscope shows that the crystals grew on, and extend above, the polished surface of the specimen. This surface is broken by pits or craters that represent the source of some of the material incorporated in the crystals.

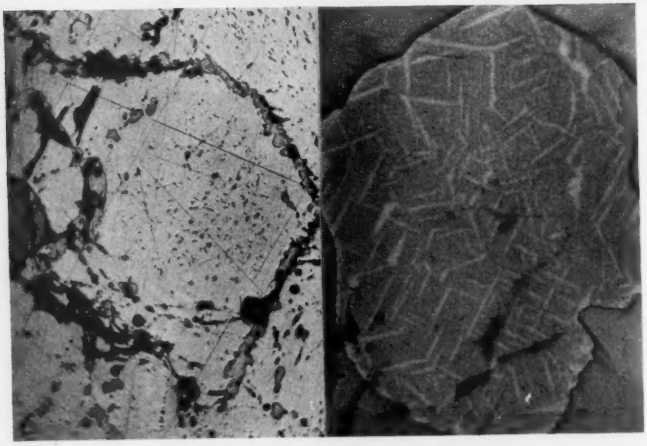
All the crystals are too small to permit accurate measurements (Fig. 5). The principal faces are the basal pinacoid and an arrangement of prisms and pyramids. The crystals have a tabular form caused by the extensive development of the basal pinacoid.

From the occurrence of the crystals at only one end of the specimen, it is assumed that, that end may have been slightly cooler. This assumption, to-gether with the fact that the crys-

[20] Ehrenberg, H.: *Op. cit.*



FIGS. 2–5



FIGS. 6 AND 7

tals grew above the polished surface, and the presence of the craters, seems to indicate that they were formed by sublimation.

The hypothesis of the formation of crystals by sublimation has been mentioned in the literature. Winchell,[21] Guild,[22] and Schneiderhöhn [23] describe crystals formed by sublimation in furnaces. Schwartz [24] describes both bornite and chalcopyrite crystals formed by this process. It may now be added on the basis of these experiments that pyrrhotite may also form crystals by sublimation.

Summary and Conclusions on Pyrrhotite-Pentlandite Relationships.

1. Pentlandite goes into solid solution in pyrrhotite between the temperatures of 425° and 450° C. These temperatures establish the position of the line *M–L* of Newhouse's diagram.

2. Pentlandite may be caused to unmix from pyrrhotite by

[21] Winchell, A. N.: Note on certain copper minerals. Amer. Min., vol. 28, pp. 244–246, 1901.

[22] Guild, F. N.: Bornite as a furnace product. Amer. Min., vol. 9, pp. 201–205, 1924.

[23] Schneiderhöhn, H.: Entmischungserscheinungen innerhalb von Erzmischkristallen. Metall. und Erz, vol. 19, pp. 501–508 and 517–526, 1922.

[24] Schwartz, G. M.: Intergrowths of bornite and chalcopyrite. Econ. Geol., vol. 26, pp. 186–201, 1931.

Fig. 2. Small grains in center formerly occupied by pentlandite before heating; grains are now pyrrhotite and lie between larger pyrrhotite grains. Heated and quenched. Exp. 12. × 235.

Fig. 3. Pentlandite (dark grey), unmixed from pyrrhotite by slow cooling, is segregated into rounded masses along pyrrhotite (light grey) grain boundaries. Exp. 13. × 95.

Fig. 4. Section of Fig. 6 at higher magnification. The small lenses within the pyrrhotite (light grey) are pentlandite. Exp. 13. × 200.

Fig. 5. Hexagonal pyrrhotite crystals formed by sublimation, on polished surface during heating. Exp. 1. × 75.

Fig. 6. Pentlandite (medium grey) unmixed from pyrrhotite by slow cooling, around a pyrrhotite (light grey) grain. Exp. 13. × 85.

Fig. 7. Pentlandite grain, containing oriented laths of pyrrhotite, unmixed from the surrounding pyrrhotite by slow cooling. The pyrrhotite in laths does not photograph the same color as the pyrrhotite which surrounds the pentlandite grain. Exp. 13. × 1400.

slow cooling from 800° C. The unmixed pentlandite then segregates around the pyrrhotite grain boundaries. This agrees with Van der Veen's contention that such textures are the product of exsolution.

3. The unmixed pentlandite contains oriented laths of pyrrhotite. This may be because either the boundary of the field of liquid immiscibility (*A* in Fig. 1) does not extend to the 100 per cent pentlandite line or the liquids in the field are partially miscible, thus allowing some pyrrhotite to disolve in pentlandite and later unmix at a lower temperature. An alternative hypothesis is the break-down of pentlandite expelling pyrrhotite.

4. The scarcity of natural intergrowths may be explained by the assumption that unmixing takes place at temperatures where diffusion is rapid and the segregation of the minerals is complete. The difficulty in finding a suitable contact plane in the atomic structures of the minerals may also be a factor.

5. Crystals of pyrrhotite were formed on specimens by sublimation.

6. The limited solubility of pentlandite in pyrrhotite makes it seem improbable that all pentlandite is a product of exsolution, but probable, that large masses are produced by crystallization from a field of immiscible liquids. Small pentlandite grains around pyrrhotite grain boundaries and pentlandite lenses within pyrrhotite grains are the product of unmixing.

PYRRHOTITE-CHALCOPYRITE RELATIONSHIPS.

Chalcopyrite is the metallic sulphide most universally associated with pyrrhotite. Several surveys of the literature have been made by different authors and the paragenesis of pyrrhotite and chalcopyrite has been established. One of the earliest of these surveys was made by Graton and Murdock [25] who concluded that chalcopyrite is generally later than pyrrhotite. Thompson,[26] after studying the ores of several copper deposits, gives as his order of

[25] Graton, L. C., and Murdock, J.: The sulphide ores of copper. A. I. M. E., vol. 45, pp. 26–81, 1913.

[26] Thompson, A. P.: The relation of pyrrhotite to chalcopyrite and other sulphides. ECON. GEOL., vol. 9, pp. 153–174, 1914.

paragenesis: (1) pyrite, (2) sphalerite, (3) pyrrhotite, (4) chalcopyrite. Schwartz [27] states that a close relationship exists between chalcopyrite and pyrrhotite, with the minerals forming contemporaneously in part, but with the bulk of the chalcopyrite somewhat later than the pyrrhotite.

The textures produced by pyrrhotite and chalcopyrite have been recently reviewed by Schwartz.[28] He states that chalcopyrite occurs as veinlets in pyrrhotite; as replacements in pyrrhotite and as alternating bands with pyrrhotite. Goodchild [29] and Schwartz [30] describe oriented intergrowths of chalcopyrite in pyrrhotite. Such evidence indicates a close relation between the two minerals. This is further supported by the work of Merwin and Lombard [31] in their investigations on the System, Cu-Fe-S. They show that under suitable conditions pyrrhotite and chalcopyrite may react to form cubanite. This relation is also illustrated in a series of experiments by Borchert,[32] who discusses the formation of cubanite from pyrrhotite and chalcopyrite and also mentions two uncommon minerals, chalcopyrrhotite and valeriit, which may be derived in a like manner. He states that pyrrhotite slowly dissolves in chalcopyrite above 300° C., forming a solid solution which on cooling forms an intergrowth of chalcopyrrhotite and chalcopyrite. Chalcopyrrhotite is stable down to 255° C. but below this temperature, upon slow cooling, it will separate into valeriit and chalcopyrite. The valeriit as inclusions in chalcopyrite is stable only down to 225° C. below which it loses CuS to chalcopyrite, and pyrrhotite remains. Schwartz [33] concludes that

[27] Schwartz, G. M.: The paragenesis of pyrrhotite. ECON. GEOL., vol. 32, pp. 31–55, 1937.

[28] Idem.

[29] Goodchild, W. H.: The origin and occurrence of crystallographic intergrowths. ECON. GEOL., vol. 11, pp. 397–402, 1916.

[30] Schwartz, G. M.: A sulphide diabase from Cook County, Minnesota. ECON. GEOL., vol. 20, pp. 201–205, 1925.

[31] Merwin, H. E., and Lombard, R. H.: The system, Cu-Fe-S. ECON. GEOL., vol. 32, pp. 203–284, 1937.

[32] Borchert, H.: Uber Entmischungen im system Cu-Fe-S und ihre Bedeutung als geologische Thermometer. Chemie Der Erde, vol. 9, pp. 145–172, 1935.

[33] Schwartz, G. M.: Intergrowths of chalcopyrite and cubanite. ECON. GEOL., vol. 22, pp. 44–61, 1927.

cubanite occurs only in close association with chalcopyrite and pyrrhotite.

Description of the Experiments.

Material.—The Parry Sound, Ontario, specimens selected for the following experiments consisted entirely of massive pyrrhotite and chalcopyrite, although other minerals were present. The pyrrhotite was in irregular masses most of which were several times longer than wide. The chalcopyrite was interstitial, and it was the predominant mineral in most of the specimens, making up about 70 per cent of the whole.

Table II lists the experiments on chalcopyrite and pyrrhotite in the order in which they were carried out. The temperature, time and length of cooling is given for each. Any changes in the specimens after heat treatment are noted in the table. Experiments in which the product of another experiment is used are shown.

Observations on the Experiments.—The work on pyrrhotite and chalcopyrite was carried on at temperatures ranging from 350° to 900° C. The length of heating for most experiments was 48 hours, but some were run in as short a time as 6½ hours.

The first problem was to determine whether or not pyrrhotite would dissolve in chalcopyrite. Many pyrrhotite areas were greatly reduced in size after heating and the textures showed that chalcopyrite now occupied positions formerly held by pyrrhotite. This was true in all the experiments from 350° to 900° C. Pyrrhotite dissolved in chalcopyrite by heating for only 6½ hours at 500° C.

Experiments near 900° C.—The behavior of the minerals in the neighborhood of 800°–900° C. was next determined. At 900° C. the specimens began to fuse. The internal structure of the cooled material was wholly changed. The previously irregular areas of pyrrhotite changed to well rounded areas and large lenses of chalcopyrite extend through the formerly homogeneous pyrrhotite. These lenses are many times longer than wide and some are a little rough on the edges. In any one specimen, most of the lenses are of the same maximum thickness. The lenses

TABLE II.

EXPERIMENTS ON PYRRHOTITE AND CHALCOPYRITE.

Exp.	Temp.	Time	Time of Cooling	Observations
C–1.	500° C.	50 hrs.	5 min.	Some pyrrhotite dissolved in chalcopyrite. Pyrr. and chal. crystals grew on surface.
C–2.	500° C.	20 hrs.	6 hrs.	A part of the product of No. C–1 was used. Pyrr. continued to go into solution. An aureole of chalcopyrite-chalcopyrrhotite intergrowth developed around pyrr. masses.
C–3.	900° C.	48 hrs.	5 hrs.	The sample fused. On cooling chal. arranged itself in parallel lenses in pyrr. Chalcopyrrhotite and bornite formed intergrowths with chal.
C–4.	800° C.	20 hrs.	6 hrs.	A part of the product of No. C–3 was used. The chal. segregated outside of the pyrr. grains leaving no trace of the intergrowth formed in No. C–3.
C–5.	800° C.	46 hrs.	5 min.	Chal. formed thin lenses in an intergrowth with pyrr.
C–6.	800° C.	24 hrs.	24 hrs.	A part of the product of No. C–5 was used; no change.
C–7.	600° C.	48 hrs.	5 min.	Some pyrr. dissolved in chal. A triangular intergrowth of chal. and chalcopyrrhotite was formed in aureoles around pyrr. on cooling.
C–8.	600° C.	18 hrs.	12 hrs.	A part of the product of No. C–7 was used. Chal. unmixed in little rounded masses from pyrr.
C–9.	580° C.	48 hrs.	5 min.	Some pyrr. dissolved in chal. Same intergrowth as in No. C–7 in aureoles.
C–10.	550° C.	48 hrs.	5 min.	Some pyrr. dissolved in chal.
C–11.	500° C.	48 hrs.	5 min.	Some pyrr. dissolved in chal. Same intergrowth as in No. C–7 was formed on cooling.
C–12.	450° C.	48 hrs.	5 min.	Part formed an intergrowth of chal. and chalcopyrrhotite. Crystals of chal. and pyrr. grew on surface.
C–13.	500° C.	20 hrs.	6 hrs.	A part of the product of No. C–12 was used. The chalcopyrrhotite spindles enlarged through slow cooling.
C–14.	400° C.	50 hrs.	5 min.	Some diffusion of pyrr. into chal. in small areas around pyrr. masses forming aureole of chalcopyrite-chalcopyrrhotite intergrowth. Crystals of chal. and pyrr. grew on surface.
C–15.	350° C.	48 hrs.	5 min.	Same results as in C–14.
C–16.	700° C.	48 hrs.	5 min.	Intergrowths of chal. and pyrr. and chal. and chalcopyrrhotite were formed. Crystals of pyrr. and chal. grew on surface.
C–17.	750° C.	48 hrs.	5 min.	Same results as in C–16.
C–18.	500° C.	25 hrs.	5 min.	Some pyrr. dissolved in chal. and cooled to chalcopyrite-chalcopyrrhotite intergrowth. Same crystals as in C–16.
C–19.	500° C.	12 hrs.	5 min.	Same as C–18.
C–20.	500° C.	6½ hrs.	5 min.	Same as C–18.

on a polished surface are oriented in only one direction in a pyrrhotite grain. The chalcopyrite also formed rounded masses at places in the pyrrhotite (Fig. 12).

A thorough examination of the chalcopyrite lenses intergrown in the pyrrhotite failed to disclose any intergrowths such as occur in pentlandite grains in pyrrhotite (Fig. 7). Slow cooling from 800° C. down to room temperature caused the chalcopyrite lenses in the pyrrhotite to migrate into the chalcopyrite that surrounded the pyrrhotite. This left the specimen a mass of granular pyrrhotite and chalcopyrite, devoid of any intergrowths.

In specimens heated between 600° and 700° C. the lenses of chalcopyrite in pyrrhotite are very minute, but nearly all are oriented in one direction. None were observed in any experiment below 600° C.

The chalcopyrite that surrounded the rounded pyrrhotite areas after heating, was not homogeneous; but under the oil immersion lens proved to be an intergrowth. This intergrowth is of the lamellar type and is extremely fine. The mineral with which chalcopyrite is intergrown has a pale green color in comparison with pyrrhotite and appears light brown compared with chalcopyrite. It is very slightly anisotropic although in many places it was thought to be isotropic.. This description fits the mineral chalcopyrrhotite, described by Borchert.[34] The physical similarity, supplemented by a close comparison of the intergrowths produced in this work with those in Borchert's photographs, leaves no reasonable doubt that the mineral is chalcopyrrhotite (Figs. 8, 9, 11).

In one specimen, an intergrowth of bornite and chalcopyrite formed around the edges of a mass of chalcopyrite that contained no pyrrhotite (Fig. 13). A deposit of sulphur developed on the inside of the tube in this experiment.

Experiments below 600° C.—In all of the experiments at temperatures below 600° C. some of the pyrrhotite dissolved, reacted with the chalcopyrite, and produced the chalcopyrite-chalcopyrrhotite intergrowth. The intergrowth does not occur

[34] Borchert, H.: *Op. cit.*

throughout the chalcopyrite as it does in specimens heated above 600° C., but forms an aureole around the various pyrrhotite areas (Fig. 10). These aureoles are slightly darker than the chalcopyrite. Fig. 11, which is an oil immersion photograph, shows that the aureoles are actually intergrowths. It also shows the chalcopyrite fingering into the intergrowth.

Small crystals of pyrrhotite and chalcopyrite grew on the surface of the specimens in nearly all of the experiments from 400° C. upward.

Interpretation of the Results.

Application of the Data on the System, Cu-Fe-S to Pyrrhotite-Chalcopyrite Relations.—The results of the foregoing experiments can perhaps be related to the Cu-Fe-S system, worked out by Merwin and Lombard.[35] The diagram which they supply with their paper is not a melting diagram but shows the reaction of solids in the presence of sulphur vapor under pressure. The present investigation, which deals with the formation and exsolution of solid solutions in the absence of sulphur vapor, can not be interpreted directly by Merwin and Lombard's diagram as it does not show the phase changes in the solid state during cooling. A speculation as to these phase changes, however, may be made. Merwin and Lombard[36] state that if a chalcopyrite melt loses sulphur the resulting melt, upon cooling, will form a solid solution. With this in mind, consider the phase relations at the chalcopyrite end of a pyrrhotite-chalcopyrite melt. Since the present work was carried on in the absence of an atmosphere of sulphur vapor, any reaction between chalcopyrite and pyrrhotite would result in a loss of sulphur. This would move the composition of the chalcopyrite into the area of solid solutions. Iron from pyrrhotite would tend to move the composition of the resulting solid solution toward pyrrhotite. Chalcopyrrhotite has a composition between chalcopyrite and pyrrhotite and slow cooling of this solid solution might result in the development of a chalcopyrite-chalcopyrrhotite intergrowth by unmixing.

[35] Merwin, H. E., and Lombard, R. H.: *Op. cit.*

[36] Idem.

If the phase relations at the pyrrhotite end of a pyrrhotite-chalcopyrite melt are considered, the area of pyrrhotite solid solutions, experimentally determined by Merwin and Lombard,[37] indicates that pyrrhotite solid solutions do exist. These solid solutions, under the proper conditions of cooling, unmix to form intergrowths of chalcopyrite and pyrrhotite, such as were produced in this work.

The bornite produced in the present experiments involves a loss of sulphur and iron from chalcopyrite. The loss in sulphur may be accounted for by a deposit of sulphur which was found on the inside of the tube in this experiment. The bornite-chalcopyrite intergrowth developed around the edge of the specimen, in the center of which the chalcopyrite-chalcopyrrhotite intergrowth formed. It is thought that iron migrated from the chalcopyrite, at the edge of the specimen, to help form the chalcopyrrhotite in the center. This may have reduced the iron content to the point where the copper content in the solid solution was high enough to produce bornite. Slow cooling caused the solid solution to unmix, forming the bornite-chalcopyrite intergrowth.

Textures.—The textures shown by the intergrowths formed in these experiments may be discussed under three headings: (1) Chalcopyrite-chalcopyrrhotite textures, (2) Pyrrhotite-chalcopyrite textures, (3) Bornite-chalcopyrite textures.

Chalcopyrite-Chalcopyrrhotite Textures.—The textures as viewed on a polished surface in the chalcopyrite-chalcopyrrhotite intergrowths may be classified as the triangular lattice type. Blades of chalcopyrite are oriented in three directions in a matrix of chalcopyrrhotite. The blades are narrowest where they intersect. Small blades of chalcopyrite are oriented in two or three directions between the larger blades (Fig. 9). Fig. 11 is a photograph of the contact of a chalcopyrite-chalcopyrrhotite aureole with chalcopyrite. It shows how the chalcopyrite fingers into the intergrowth. Borchert[38] gives illustrations of chalcopyrite-chalcopyrrhotite intergrowths identical to these. Schwartz[39]

[37] Idem.

[38] Borchert, H.: *Op. cit.*

[39] Schwartz, G. M.: Textures due to unmixing of solid solutions. ECON. GEOL., vol. 26, pp. 739–763, 1931.

gives illustrations of similar textures in his discussion on the unmixing of solid solutions.

The planes along which chalcopyrite is oriented in chalcopyrrhotite are not known; but Schwartz,[40] in describing a similar intergrowth of chalcopyrite and cubanite, states that the cubanite is oriented along the (111) planes in chalcopyrite.

Pyrrhotite-Chalcopyrite Textures.—The intergrowth formed by chalcopyrite and pyrrhotite is noteworthy in that few natural intergrowths of these minerals have been described. Schwartz [41] notes such an intergrowth in a sulphide diabase from Cook County, Minnesota, and considers it to be the product of unmixing. The texture of the intergrowths produced in this work consists of long, narrow lenses of chalcopyrite generally oriented in one direction in the pyrrhotite. The lenses are remarkably uniform over a large area but minor irregularities occur locally along their sides. Few are in contact with one another (Fig. 12).

Gruner [42] states that chalcopyrite is oriented along the (0001) plane in pyrrhotite. Scholtz [43] notes that the (0001) plane of pyrrhotite is a prominent parting plane and that it is the principal plane along which crystallographic intergrowths in pyrrhotite are oriented.

Bornite-Chalcopyrite Textures.—The bornite-chalcopyrite intergrowth formed in these experiments is much like the bornite-chalcopyrite intergrowth produced by Schwartz.[44] It is a lattice type of intergrowth that has the chalcopyrite blades oriented, in places, in a rudely triangular pattern. The chalcopyrite blades are narrowest at intersections as is typical of exsolution textures. Schwartz [45] states that this type of intergrowth is characteristic of specimens formerly predominantly chalcopyrite, as the relative amounts of the minerals in the specimen govern the form of the

[40] Schwartz, G. M.: Intergrowths of chalcopyrite and cubanite. *Op. cit.*
[41] Schwartz, G. M.: A sulphide diabase from Cook County, Minn. *Op. cit.*
[42] Gruner, J. W.: *Op. cit.*
[43] Scholtz, D. L.: *Op. cit.*
[44] Schwartz, G. M.: Intergrowths of bornite and chalcopyrite. *Op. cit.*
[45] Idem.

intergrowth. Gruner [46] states that the contact planes of this intergrowth are the (111) planes in both chalcopyrite and bornite.

Crystals.—Small crystals of chalcopyrite and pyrrhotite grew on the surfaces of the specimens in nearly all of the experiments. They were formed at temperatures as low as 400° C. and in as short a time as 6½ hours. The crystals do not extend below the surface of the specimen. The surface between the crystals had numerous pits or craters in it, similar to those described under the pyrrhotite-pentlandite section of this paper.

The pyrrhotite crystals are much the same as those already described, but the chalcopyrite crystals are commonly twinned and show many faces. Most of the crystals occur so closely packed together, that they interfere.

The close resemblance of these crystals and their environment to those formed in the pyrrhotite-pentlandite experiments indicates a similar origin by sublimation. Schwartz [47] produced similar chalcopyrite crystals and concluded that they were products of sublimation.

Summary and Conclusions on Pyrrhotite-Chalcopyrite Relationships.

1. Pyrrhotite is the predominant mineral in a solid solution of pyrrhotite and chalcopyrite, which forms above 600° C.

2. Chalcopyrite is the predominant mineral in a solid solution of pyrrhotite and chalcopyrite which forms between 300° and 900° C.

3. Chalcopyrrhotite is produced either by the diffusion of pyrrhotite through chalcopyrite or by the unmixing of a solid solution of pyrrhotite in chalcopyrite, in which the pyrrhotite has reacted with the chalcopyrite. In the first case the chalcopyrrhotite is restricted to an aureole of chalcopyrite-chalcopyrrhotite intergrowth around pyrrhotite masses, and in the second case the chalcopyrrhotite may be found throughout the chalcopyrite.

4. The chalcopyrite-chalcopyrrhotite intergrowth is a typical

[46] Gruner, J. W.: *Op. cit.*

[47] Schwartz, G. M.: *Op. cit.*

triangular lattice intergrowth and may form upon cooling from temperatures between 400° and 900° C.

5. Chalcopyrite forms long, narrow, parallel lenses in the pyrrhotite-chalcocopyrite intergrowth which may be produced by cooling suitable material from temperatures above 600° C.

6. Bornite was produced in one experiment from chalcopyrite through a loss of sulphur and iron, by heating the specimen at 900° C. This bornite formed a lattice intergrowth with chalcopyrite.

7. Crystals of chalcopyrite and pyrrhotite were formed by sublimation at temperatures as low as 400° C.

PYRRHOTITE-GALENA RELATIONSHIPS.

Although galena is not considered a characteristic high temperature mineral it is commonly associated with pyrrhotite in minor amounts. Campbell and Knight [48] describe galena, in veins with sphalerite, chalcopyrite and quartz, cutting pyrrhotite at Mount Nickel. Waldshmidt [49] describes pyrrhotite strung out in galena flowage ores, and broken pyrrhotite cemented by galena in East Hecla ores. Schwartz [50] lists 14 districts in which the paragenesis of pyrrhotite and galena is given and in every case the galena is later than the pyrrhotite. A review of the literature fails to show any reference to intergrowths between pyrrhotite and galena. The fact that galena is probably always later than pyrrhotite may account for their absence. An eutectic [51] exists in the artificial binary system PbS-FeS and forms intergrowths.

Description of the Experiments.

Material and Its Preparation.—The material used in Experiments S–1 to S–10 inclusive as well as Experiments S–12 and S–17 was ore from the Sullivan Mine, Kimberly, B. C. This was the material in which the intergrowth was first encountered

[48] Campbell, W., and Knight, C. W.: *Op. cit.*

[49] Waldshmidt, W. A.: Deformation of ores, Coeur d'Alene district, Idaho. ECON. GEOL., vol. 20, pp. 573–586, 1925.

[50] Schwartz, G. M.: The paragenesis of pyrrhotite. *Op. cit.*

[51] International Critical Tables, vol. 4, pp. 52.

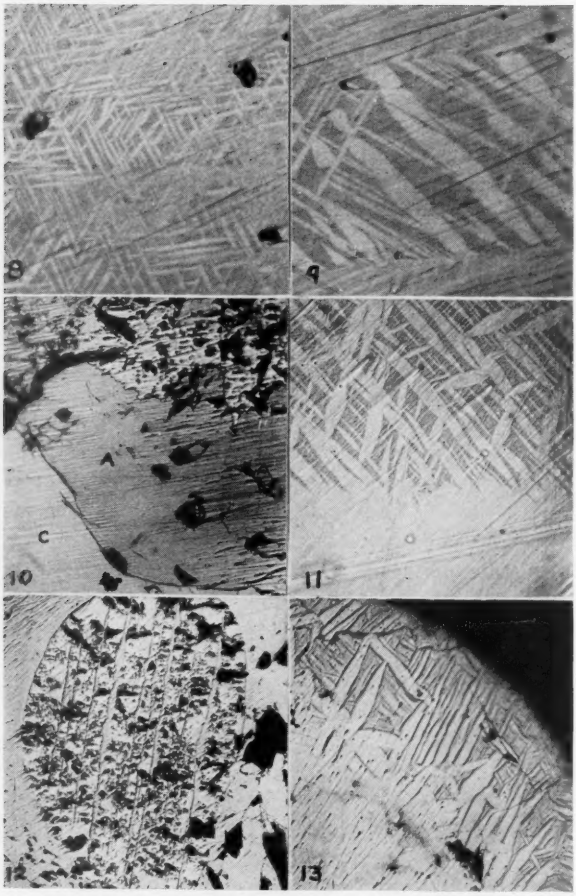


FIG. 8. Chalcopyrite (light grey) blades oriented in three directions in chalcopyrrhotite. Specimen was quenched after heating. Exp. C–7. × 1000.

FIG. 9. Chalcopyrite-chalcopyrrhotite intergrowths, with probably three grains shown; triangular type in upper and lower grains, bladed type in center. Specimen was quenched after heating. Exp. C–7. × 1000.

after heat treatment and consists entirely of sphalerite, galena and pyrrhotite. Sphalerite is the predominant mineral in this material. Galena is second in abundance and contains obviously corroded remnants of both pyrrhotite and sphalerite. It fills in and rounds off irregularities in both pyrrhotite and sphalerite and thus seems to be the latest of the three minerals. Pyrrhotite occurs as irregular grains and as elongated lath-like areas, which penetrate the sphalerite either along grain boundaries or along cleavages, indicating contemporaneous deposition.[52] The material for the above experiments was prepared in the usual manner.

Experiment S–11 was conducted with coarse pyrrhotite and galena of high purity but unknown source. A series of 5 experiments (S–13, S–14, S–15, S–16 and S–18) were run with mixtures of similar pure pyrrhotite and galena ground to 80 mesh. The proportions of pyrrhotite and galena desired in the experiment were weighed out, thoroughly mixed, and put in a steel mould equipped with a close-fitting plunger and subjected to 10,000 lbs. pressure. This compressed the powder into easily handled pellets.

Table III lists the experiments on pyrrhotite and galena in the

[52] Schwartz, G. M.: Microscopical features of the Sullivan ore. Eng. and Min. Jour., vol. 122, pp. 375–377, 1926.

FIG. 10. Small part of a chalcopyrite-chalcopyrrhotite aureole (*A*), pyrrhotite (*P*), and chalcopyrite (*C*); *P* and *C* are much larger than *A*. Specimen quenched after heating. Contacts emphasized by ink dashes. Exp. C–15. × 295.

FIG. 11. Contact of chalcopyrite-chalcopyrrhotite aureole, shown in Fig. 10, under higher magnification; chalcopyrite (medium grey). Note that chalcopyrrhotite (dark grey) forms the interstitial material in the intergrowth and the chalcopyrite forms the blades. Exp. C–13. × 530.

FIG. 12. Rounded masses of pyrrhotite (light grey) enclose long oriented lenses of chalcopyrite (slightly darker) and are surrounded by chalcopyrite. Specimen quenched after heating. Exp. C–3. × 180.

FIG. 13. An intergrowth of bornite (dark grey) and chalcopyrite (light grey) around the edge of a chalcopyrite specimen that was quenched after heating. Exp. C–3. × 320.

order in which they were performed. The temperature, time and rate of cooling is given for each. A summary of the changes observed in each experiment completes the table.

TABLE III.

EXPERIMENTS ON PYRRHOTITE AND GALENA.

Exp.	Temp.	Time	Time of Cooling	Observations
S– 1.	600° C.	46 hrs.	5 min.	Specimens unchanged.
S– 2.	650° C.	48 hrs.	5 min.	Specimens unchanged.
S– 3.	900° C.	50 hrs.	5 min.	Specimen fused; a eutectic intergrowth between pyrrhotite and galena formed around pyrr. areas. Crystals of gal. formed on inside of tube.
S– 4.	850° C.	2 hrs.	25 hrs.	A part of product of S–3 was used; specimen fused; intergrowth became slightly coarser.
S– 5.	900° C.	47 hrs.	5 min.	Specimen fused. A coarse intergrowth of pyrr. and gal. was produced. Crystals of gal. formed on inside of tube.
S– 6.	900° C.	45 hrs.	5 min.	Same as in S–5.
S– 7.	650° C.	47 hrs.	5 min.	Specimen unchanged.
S– 8.	800° C.	48 hrs.	5 min.	Specimen fused. The same intergrowth as in S–5 formed. Crystals of gal. formed on inside of tube.
S– 9.	700° C.	48 hrs.	5 min.	Specimen unchanged. A few crystals of galena formed on inside of tube.
S–10.	775° C.	48 hrs.	5 min.	Specimen fused slightly. Same intergrowth as S–5.
S–11.	900° C.	48 hrs.	5 min.	Coarse pyrr. and gn. were used. They fused and ran together. Same intergrowth as in S–5.
S–12.	750° C.	48 hrs.	5 min.	Specimen unchanged.
S–13.	800° C.	48 hrs.	5 min.	A pellet of powdered pyrr. and gal. in the proportions, 53 per cent pyrr., 47 per cent gal. by weight was used; pellet fused. An intergrowth similar to S–5 was formed.
S–14.	800° C.	48 hrs.	5 min.	A pellet of 40 per cent pyrr. and 60 per cent gn. by weight was used; specimen fused. Same intergrowth as in S–5. A few gal. crystals formed on inside of tube.
S–15.	800° C.	48 hrs.	5 min.	A pellet of 65 per cent. pyrr. and 35 per cent gal. by weight was used. The pellet fused. Same intergrowth as in S–5 formed around masses of excess pyrr.
S–16.	800° C.	48 hrs.	5 min.	A pellet of 40 per cent pyrr. and 60 per cent gal. by weight was used; bomb placed vertically in furnace; pellet fused. Same intergrowth as in S–5 formed. Some gal. volatilized and deposited at upper end of tube forming skeletal crystals.
S–17.	765° C.	48 hrs.	5 min.	Sullivan ore was used; specimen unchanged.
S–18.	820° C.	48 hrs.	5 min.	A pellet of 15 per cent pyrr. and 85 per cent gal. was used; pellet fused. Gal. crystals formed on inside of tube.

Observations on the Experiments.—This series of experiments was conducted at temperatures ranging from 600° to 900° C. The average time for heating was 48 hours. Only one sample was cooled slowly as the intergrowth formed just as well when the specimen was quenched.

The first two experiments at 600° and 650° C. respectively were of an exploratory nature and the specimens remained unchanged. A series of experiments were next run between 700° and 900° C. The specimens heated to 900° C. lost their shape completely and became filled with gas cavities. The pyrrhotite areas in this material became well rounded and surrounding each is a pinkish-white area. This area was seen, under higher magnifications, to be an intergrowth of pyrrhotite and a white mineral which answers to the description and etch tests of galena. The intergrowth had a typical wormy-appearing eutectic pattern. Evidence of fusion as shown by corroded pyrrhotite masses and rearrangement of the galena is present wherever the intergrowth was produced. Small cubic crystals, which possess a brilliant metallic luster, grew on the inside walls of the bomb in all these experiments. The crystals were identified by chemical tests as galena.

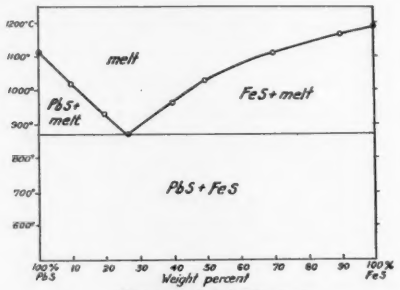
It was decided to determine the eutectic temperature of the galena-pyrrhotite system by a method of trial and error. The eutectic point lies between the temperatures 765° and 775° C., when Sullivan ore is used.

A coarse piece of pyrrhotite and one of galena were put in a bomb with about ½ an inch between them in Experiment S–11. The galena fused and ran into the pyrrhotite. The pyrrhotite lost its shape only at the end exposed to galena. The eutectic intergrowth formed in the pyrrhotite specimen and progressed along definite directions in the pyrrhotite.

A series of experiments were carried out with powdered pyrrhotite and galena to ascertain whether or not the intergrowth could be formed from these artificial mixtures, and if so, to check on the approximate composition of the eutectic. Photographs were taken of the intergrowth and several Rosiwal analyses made

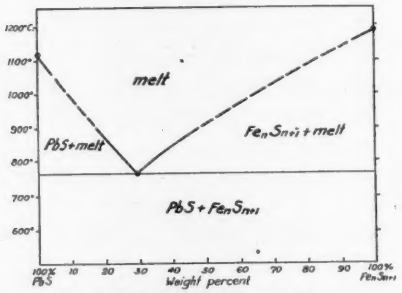
of the most common patterns. The composition, as determined by these analyses, was 60 per cent galena and 40 per cent pyrrhotite by volume, or 71 per cent galena and 29 per cent pyrrhotite

Figure 14



Freezing Point Diagram for PbS-FeS

Figure 15



Equilibrium Diagram for Pyrrhotite-Galena

by weight. Pellets of the powdered, mixed, minerals were then made up in the way that has been described. Several experiments were run with pyrrhotite in excess of the eutectic proportions. The intergrowth formed around corroded pyrrhotite grains.

Some galena volatilized in all these experiments and formed crystals at the cooler end of the bomb.

Interpretation of Results.

Application of the Freezing Point Diagram for PbS-FeS to Results.—No reference has been found to the formation or behavior of a pyrrhotite-galena intergrowth in nature. The Freezing Point Diagram for PbS-FeS as given by the International Critical Tables is, however, a valuable aid in interpreting the behavior of the system PbS-Fe_nS_{n+1}.

Fig. 14 is a diagram of the system PbS-FeS. The freezing point curve has been well established as is shown by the number of points on the curve which have been worked out. PbS and FeS exist as liquids above the curve. Two fields exist below the curve and above the eutectic temperature. A mixture of solid PbS and melt occupies one field, and a mixture of solid FeS and melt the other. The field which lies below the eutectic point consists of solid PbS and FeS in the proportions of 27 per cent FeS and 73 per cent PbS by weight. The eutectic temperature is 869° C. No information is given as to whether FeS and PbS are soluble in each other in the solid state.

Fig. 15 is a diagrammatic representation of the results obtained in this work. The melting points of galena and pyrrhotite are given in the International Critical Tables. The eutectic point as established here, lies between 765° and 775° C. The composition of the intergrowth as shown by Rosiwal analyses was calculated to 29 per cent pyrrhotite and 71 per cent galena by weight. The fields of this diagram are similar to those of Fig. 14.

A comparison of Figs. 14 and 15 shows a noteworthy difference in the temperature of the eutectic point. The compositions of the two eutectics are also different. A speculation may be offered as to the reason for these differences. It is thought by the writer that impurities in galena such as small amounts of nickel or zinc might cause the lowering of the eutectic temperature from 869° to 770° C. Excess sulphur in pyrrhotite over

the composition FeS might also have some effect. Similarly these components might shift the eutectic composition toward the pyrrhotite end of the diagram. Dix and Heath [53] in an investigation of Al-Si alloys found that the presence of "modifiers" or impurities such as sodium in the Al-Si melt would cause a lowering of the apparent euectic temperature. These impurities also changed the composition of the eutectic.

Textures.—The textures developed in these experiments are typical eutectic patterns. Fig. 16 shows the intergrowth in Sullivan ore after heat treatment. Large areas of galena and sphalerite may be seen but all the pyrrhotite has gone into the intergrowth. Fig. 17 is a section of this intergrowth more highly magnified. The round and oval areas in the intergrowth may represent centers at which crystallization first began. Desch [54] states that the crystallization of an eutectic may not begin simultaneously all over the field but may start at a number of centers. Green [55] classifies similar patterns as spheroidal.

Another less common pattern is shown in Fig. 18. No excess of either constituent appears. The dark spots on the pyrrhotite are deposits of some kind formed by the effect of light on the mineral while the photograph was being taken. This deposit may be sulphur expelled from the pyrrhotite by the influence of the light.

The texture produced by heating pieces of coarse pyrrhotite and galena together in the same bomb, but not in contact with each other until the galena fused, is shown in Fig. 19. Desch [56] and Van der Veen [57] give illustrations of a copper-silver alloy that are similar to this pattern. Desch calls it arborescent or fern-like. The intergrowth developed between rounded masses of excess pyrrhotite is also aborescent.

Fig. 20 shows a pattern produced from a mixture of powdered

[53] Dix, E. H., and Heath, A. C.: Constitution of Al-Si alloys. National Metals Handbook, 1936 Edition.

[54] Desch, C. H.: Metallography, p. 185, 1910.

[55] Green, C. H.: Eutectic patterns in metallic alloys. A. I. M. E., Trans., Feb. 1925.

[56] Desch, C. H.: *Op. cit.*

[57] Van der Veen, R. W.: Mineragraphy and ore deposition, vol. 1, 1925.

pyrrhotite and galena close to the eutectic composition. It is very similar to the intergrowth shown in Fig. 17, which was produced from Sullivan ore.

The striking similarity between these patterns and known eutectic textures is suggestive evidence that they were formed at an eutectic point.

Crystals.—Crystals of galena were formed in all the experiments carried out at temperatures of 700° C. and over. These crystals did not form on the surface of the specimens but on the inside walls of the silica glass bomb. Pits and craters similar to those described under pyrrhotite and chalcopyrite were observed. Two experiments were run with the furnace in a vertical position and, in both of them, crystals grew at the top of the bomb. This indicates that the volatilized lead sulphide rose to the top of the bomb where it cooled and deposited as crystals.

The crystals are small and mostly cubic. Small aggregates of of cubes were observed, and cubes distorted in one direction are common. Most of the crystals have a skeletal appearance due to pits on all the faces This seems to indicate that the crystals started growing along the 3 crystallographic axes and later filled in the faces of the cubes. Growth was evidently arrested before the cube faces were completely filled out, leaving pits.

The formation of these crystals at a considerable distance from the specimen and at a probably cooler end of the tube seems to be good evidence for their formation by sublimation.

Summary and Conclusions on Pyrrhotite-Galena Relationships.

1. A eutectic intergrowth was produced by heating (1) Sullivan ore, (2) pieces of coarse pyrrhotite and galena, and (3) pellets of pyrrhotite and galena in various proportions.

2. The eutectic temperature lies between 765° and 775° C. This is considerably lower than the eutectic point in the system PbS-FeS and this discrepancy is thought to be caused by the presence of sulphur in pyrrhotite in excess of the formula FeS; or small amounts of impurities like Ni or Zn, which might lower the eutectic point.

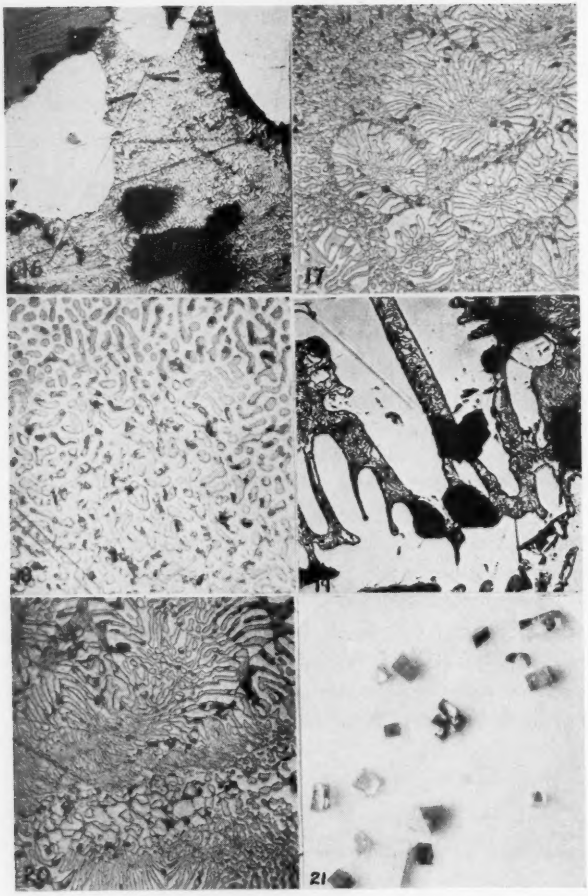


FIG. 16. Ore from Sullivan Mine, Kimberley, B. C., showing relation of pyrrhotite and galena to excess galena (white) and sphalerite (dark grey); sphalerite less attached than galena. Specimen heated and quenched. Exp. S–6. × 210.

FIG. 17. Eutectic intergrowth of pyrrhotite (medium grey) and galena (light grey) formed by heating Sullivan ore. Note spheroidal pattern. Exp. S–6. × 350.

3. The composition of the eutectic was estimated approximately as 29 per cent pyrrhotite and 71 per cent galena by weight.

4. The various patterns of the eutectic were compared with known eutectic textures and are typical of such an intergrowth.

5. Crystals of galena were produced at 700° C. and above, under conditions that strongly indicate a formation by sublimation.

GENERAL SUMMARY.

Pyrrhotite and pentlandite form a solid solution, in which pyrrhotite is the dominant mineral, between 425° and 450° C. This solid solution unmixes when the specimens are cooled slowly from 800° C. The pentlandite that unmixes from the pyrrhotite, segregates around pyrrhotite grain boundaries and under high magnifications is seen to have small pyrrhotite laths oriented symmetrically throughout. This suggests, either that pyrrhotite and pentlandite form a second solid solution in which pentlandite is the dominant mineral, or that pentlandite breaks down expelling pyrrhotite.

Pyrrhotite and chalcopyrite form two solid solutions with pyrrhotite as the dominant mineral in one of them and chalcopyrite in the other. These solid solutions unmix forming lattice intergrowths of pyrrhotite and chalcopyrite, and chalcopyrite and chalcopyrrhotite. At temperatures below 600° C. the chalcopyrrhotite is found in aureoles around pyrrhotite masses, but at higher temperatures it is found throughout the chalcopyrite.

FIG. 18. Eutectic intergrowth of pyrrhotite (medium grey) and galena (light grey) formed by heating powdered pyrrhotite and galena. Black spots on pyrrhotite lamellæ are results of light etching. Exp. S–13. × 175.

FIG. 19. Eutectic intergrowth of pyrrhotite (medium grey) and galena (white) formed by heating a chip of pyrrhotite and one of galena together. The intergrowth corrodes an area of pyrrhotite and forms an aborescent pattern in the excess pyrrhotite. Exp. S–11. × 370.

FIG. 20. Eutectic intergrowth of pyrrhotite (medium grey) and galena (white), from heat treatment of a mixture of powdered pyrrhotite and galena. Note similarity to Fig. 17. Exp. S–15. × 175.

FIG. 21. Skeletal, isometric crystals of galena, showing hopper-shaped faces, formed by sublimation. Exp. S–16. × 21.

Pyrrhotite and galena form a eutectic intergrowth between the temperatures 765° and 775° C., which has a composition of 29 per cent pyrrhotite and 71 per cent galena by weight. The textures of the intergrowth have typical eutectic patterns.

Crystals of pyrrhotite, chalcopyrite, and galena are produced by sublimation as is shown by the facts that the crystals grow upon the surface of the specimens or the inside walls of the bombs, at a probably cooler end of the tube, and that pits and craters appear on the surface of all the specimens.

UNIVERSITY OF MINNESOTA,
MINNEAPOLIS, MINN.,
Jan. 2, 1938.

EPITHERMAL HÜBNERITE FROM THE MONITOR DISTRICT, ALPINE COUNTY, CALIFORNIA.

VINCENT P. GIANELLA.

ABSTRACT.

In the Monitor district, are small irregular gold-silver deposits, in which hübnerite is present along with minerals characteristic of the epithermal zone. The gangue is chiefly quartz and rhodochrosite. The paragenesis of the hübnerite indicates that it was deposited quite late. The enclosing Miocene lavas and breccias were extensively altered by hydrothermal solutions and, due to weathering, are vividly colored. The altered rocks are overlain by relatively fresh Pliocene volcanics. The mineralization and other geological features are similar to those accompanying many other bonanza-type gold-silver deposits in the Great Basin region.

INTRODUCTION.

HÜBNERITE was found recently in ore specimens from the Zaca mine of the Monitor district in Alpine County, California. The ores in which it occurs are characteristic of the bonanza-type, epithermal gold and silver deposits typical of the Cordilleran region. The mineral was identified at the Reno Station of the U. S. Bureau of Mines, early in 1937. Although commonly found with mineral assemblages indicating a deeper-seated origin, hübnerite has occasionally been reported from deposits formed nearer the surface. Another locality where it is found in epithermal ores should prove of interest to students of ore deposits.

LOCATION AND GENERAL GEOLOGY.

The Monitor (also known as the Loope district) and the adjoining Mogul district are situated along Monitor Creek, seven miles south of Markleeville, and a mile or more above its junction with East Fork of Carson River. Ore was found in the district shortly after the discovery of the Comstock Lode and the region was the scene of active operations from 1860 to 1880.

During that time about $1,000,000 was produced [1] largely from rich, though small, ore shoots. The value of silver exceeded that of the gold. Since 1880, mining has been carried on at intervals but on a much reduced scale and for considerable periods the mines have been idle. At present only one mine is being operated.

Lindgren,[2] in discussing the Markleeville quadrangle, describes the Monitor area as follows:

> The altered rocks comprise both breccias and massive andesite. In the central area, between Mount Bullion, Monitor, Mogul and Leviathan, the rocks have suffered extreme alteration and now appear as white, yellow, and red outcrops consisting of jaspery and chalcedonic rocks as well as kaolin. In places the rocks are rich in sulphides, principally pyrite, but also argentite and various rich silver antimonides. Zinc blende, chalcopyrite, pyrargyrite, enargite, and galena also occur. No well defined veins could be seen.

Eakle [3] comments on the conspicuous cherty, or chalcedonic, silicified masses that are common to the region, and ascribes the mineralization to hot solutions that ascended through channels in the andesite and deposited ore in silicified bodies without definite walls. In addition to the minerals mentioned by Lindgren, Eakle noted gold, chalcopyrite, polybasite, stephanite, stromeyerite, and tetrahedrite. He found that the silver minerals where characteristically associated with rhodochrosite which, upon weathering, formed oxides that blackened the rocks around ore outcrops.

In addition to the andesites and andesite breccias, mentioned in the earlier reports, there is a broad mass of felsitic, to glassy, rhyolite at the site of the Zaca mine. Good exposures may be seen at the portal of the adit where mining operations are now in progress. This mass of rock crosses the canyon at this locality and extends up the walls of the canyon and beyond, to an undetermined distance. The adit is about half a mile downstream from the site of the town of Monitor (also known as Loope).

[1] Lindgren, Waldemar: The Tertiary gravels of the Sierra Nevada of California. U. S. Geol. Surv. Prof. Paper 73, p. 191, 1911.

[2] Idem, p. 191.

[3] Eakle, A. S.: Mines and mineral resources of Alpine County, Inyo County, and Mono County. Chapters of State Mineralogist's Report, 15: 5–27 (1915–16), Cal. State Min. Bur., p. 9, 1917.

Little now remains to indicate the location of this once prosperous mining center.

The formations have a northerly strike and dip steeply to the east. The rhyolite is flow-banded and this structure is a conspicuous feature along the canyon walls, even when viewed from a considerable distance. On close inspection, the flow-banding is found to be very pronounced with as many as 30 layers to the inch. Where weathered, the rock may take on the appearance of a clay shale. In much of the rock, which has been completely replaced by ore and gangue minerals, the flow-structure is delicately preserved.

Study of a thin section of the wall-rock, in contact with the ore, reveals a strongly flow-banded vitrophyre. The rare phenocrysts are quartz, orthoclase, minor amounts of sodic plagioclase and sparse biotite now altered to clear, colorless, muscovite. Much of the feldspar has been replaced by an aggregate of quartz, or quartz and sericite. Veinlets and irregular areas of quartz, in places accompanied by sericite and rhodochrosite, replace the groundmass. The replacement is partly controlled by the flow-banding but also penetrates the rock at random without regard to structural control. Some of the fine quartz veinlets, found penetrating the wall-rock, contain ore minerals and fine clusters of minute crystals of hübnerite. These crystals are commonly acicular (Fig. 3).

A short distance downstream, below the protal of the adit, the rhyolite gives way to andesite and andesite breccias, which are the most abundant rocks of the district. Insufficient evidence was obtained to indicate whether the rhyolite is a flow or is intrusive into the andesites.

About a mile south of Markleeville the older volcanics are overlain by younger, unmineralized, andesite lavas, breccias, and coarse agglomerates. The older lavas have been altered by hydrothermal solutions, with the introduction of pyrite. Subsequent weathering, superposed upon the propylitization, has produced brilliant coloring with white and various shades of red and yellow that contrasts strongly with the dark and somber coloring

of the younger volcanics. The vivid coloring of the mineralized volcanics tends to bring into striking contrast the widespread propylitization accompanying the deposition of the small ore shoots within restricted areas. Similar extensive coloration is a characteristic of the Tertiary lavas containing epithermal ore deposits in many Nevada mining districts.

The lithology, mineralization, weathering phenomena, and sequence of the rocks in the Monitor district is strongly reminiscent of the Miocene and Pliocene volcanics of the Comstock region.[4] The younger (Pliocene) lavas cover much of the eastern flank, summit region, and western slope of the Sierra Nevada at this latitude. These lavas have been extensively described in the literature but the older Tertiary volcanics, which are common to the Great Basin region, are evidently not plentiful in the mountainous region to the west as little reference to them is to be found in the various publications covering that area.

MINERALIZATION

The Zaca mine is operated through an adit driven from Monitor Creek into Colorado Hill, for a distance of over 1,300 feet. Colorado Hill derives its name from the multi-colored rocks of the region. The adit (Lincoln tunnel of the older reports), judging from the material composing the dump, is almost entirely in rhyolite. The ore bodies occupy small fissures, lenses, and irregular masses within this rock.

In the ore bodies, dense stony quartz, granular and crystalline quartz, rhodochrosite, pyrite, galena, sphalerite, argentite, stephanite, tetrahedrite, hübnerite, sericite, iron and manganese oxides, gypsum, calcite, and clay minerals, were recognized. In some places large masses of clay are encountered in the mineralized areas, particularly in the vicinity of the ore bodies. Minor amounts of arsenopyrite have been reported but none was apparent in the ore specimens studied during this investigation. The ore minerals are associated with both quartz and rhodochrosite

[4] Gianella, V. P.: Geology of the Silver City district and the southern portion of the Comstock Lode, Nevada. Univ. Nev. Bull., vol. 30, no. 9, pp. 45–59, 68–73, 1936. Ferguson, H. G.: Econ. Geol., vol. 6, p. 859, 1937.

but more commonly with the manganese carbonate. The rhodochrosite commonly contains numerous small cavities covered with druses of clear quartz crystals. These drusy incrustations are generally free from ore minerals. This quartz is of a later

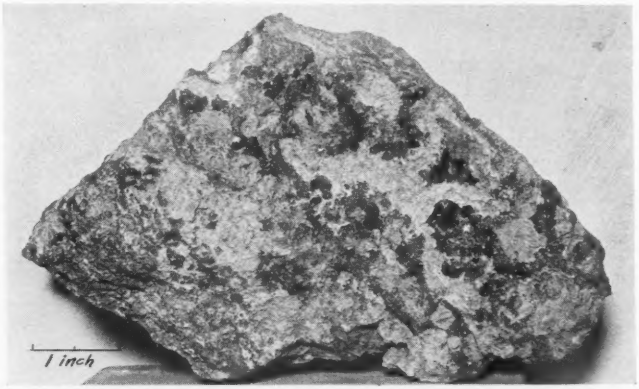


FIG. 1. Ore specimen from the Zaca mine showing rhodochrosite (light gray), clayey material (dark gray), and ore minerals (dark irregular masses); the sulphides are principally argentiferous galena, sphalerite, and pyrite. A vug, lined with quartz crystals, is shown in the rhodochrosite in the upper right.

generation than the vein quartz with which the ore minerals are associated. A typical ore specimen is illustrated in Fig. 1. Rarely, crystals of rhodochrosite are found resting upon the drusy quartz and are, in turn, partially covered with minute quartz crystals. This relationship indicates a rhythmic deposition of some of the minerals with at least three generations of quartz and two of rhodochrosite. This ore is of the bonanza-type which, aside from the presence of the hübnerite, is similar to the ores of many mines of the Great Basin region. The mineral association and type of alteration clearly indicates its deposition in the epithermal zone. The ore and gangue minerals generally parallel the flow-banding of the rhyolite preserving its delicate structure.

In many places, however, they cut across these bounding planes and replace the rock in a most irregular manner. The cross-cutting of this structure gives replaced masses, sometimes of large

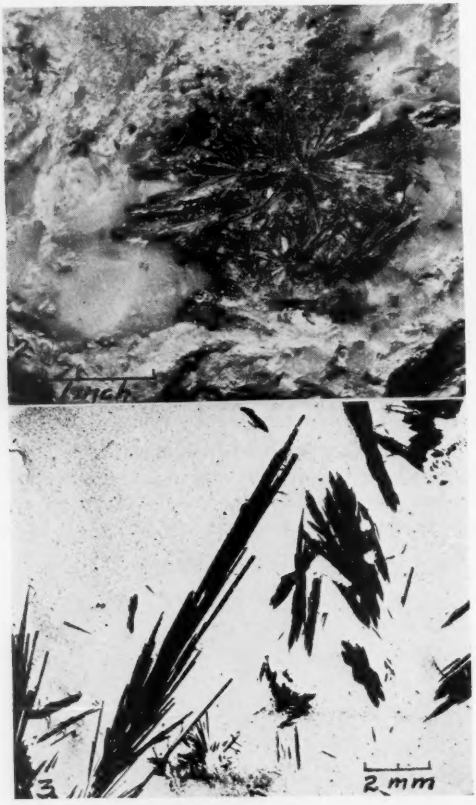


FIG. 2. Hand specimen with a large cluster of radiating crystals of hübnerite is silicified rhyolite. The translucent areas, to the right and left, consist of micro-crystalline silica.

FIG. 3. Hübnerite crystals penetrating rhyolite glass. The dark area in upper left is dark brown glass. (Plane light.)

size, without sharp boundries and these merge, with insensible gradations, into the surrounding rock.

The hübnerite is light to dark brown, reddish-brown, and dark red in color. In general it is translucent but many of the smaller reddish crystals are clear and transparent. Hübnerite is found encrusting the ore minerals and drusy quartz, in cavities within the stony quartz, and in quartz or rhodochrosite veinlets that penetrate granular quartz or cut the unreplaced wall rock. Crystals, less than a millimeter in length, were found attached to the surface of minute quartz crystals lining a vug in ore minerals. A slab from a cavity in a rich ore shoot is composed of dense stony quartz coated with fine drusy quartz. Upon the drusy surface are occasional quartz crystals up to three fourths of an inch in length. On this slab are many clusters of hübnerite crystals. The crystals are prismatic in shape and are arranged in both parallel and radiating groups. These clusters range from one fourth to one half inch wide and from one half to three fourths of an inch long. The relationships, discussed above, evidence the late introduction of the manganese tungstate mineral.

In craggy outcrops, several hundred feet up the hillside, above the tunnel, hübnerite occurs as beautiful stellate clusters, up to three inches in diameter, embedded in dense silica that has replaced the rhyolite (Fig. 2). Mr. O. J. Benstron [5] informed the writer that hübnerite is looked for because it is commonly associated with the richer ores. In places this mineral is so plentiful as to give a distinct brown band on the concentrating table. It is possible that this concentrate might prove to be a valuable by-product from the milling of the gold-silver ore. Its constant association with the higher grade ores would indicate an intimate relationship with the origin of these minerals.

In thin section, the hübnerite appears as pale yellowish-green crystals penetrating both glassy and silicified rhyolite. It is non-pleochroic. In places the crystals are so intergrown as to form irregular masses. Both slender crystals and irregular masses are shown in Figs. 3 and 4. Minute crystals are disseminated throughout the section. Where hübnerite has been introduced

[5] Mill superintendent Zaca mine. Personal communication.

the glass commonly has been devitrified and in general fine-grained crystalline quartz is abundant. In Fig. 3 crystals are seen penetrating the rhyolitic glass. Practically all of the groundmass is glass but minute grains of quartz, not apparent in the photomicrograph, are closely associated with hübnerite. In Fig. 4 hübnerite and fine-grained quartz have completely replaced

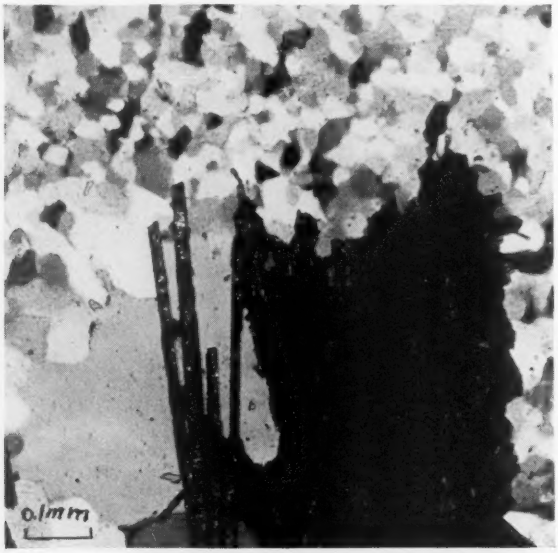


FIG. 4. Vein quartz and hübnerite in silicified rhyolite. The dark gray area, in lower left, is open field. (Crossed nicols and mica plate.)

the rhyolite. These photomicrographs were taken of a thin section from a quartz-hübnerite veinlet that penetrates silicified rhyolite in the vicinity of the cluster shown in Fig. 2. In this area the rhyolite has been much altered with the introduction of silica through hydrothermal activity. Where somewhat weathered, the hübnerite commonly has hydrous iron oxides penetrating along cleavage planes suggesting that it may have a con-

siderable content of iron. Qualitative tests made on unaltered material indicate that some of the specimens contain only a small quantity of iron and others only a trace. Through the courtesy of Professor Walter S. Palmer,[6] an analysis on fresh material was run by Professor William I. Smyth.

ANALYSIS OF HÜBNERITE FROM THE MONITOR DISTRICT, CALIFORNIA.

William I. Smyth, Analyst.

MnO	23.64%
FeO	2.38
WO_2	73.97

The analysis bears out the qualitative tests, which indicated a low iron content. Hübnerite, as defined by Hess,[7] may have up to 4.74% FeO. The iron in the Monitor material is well within this limit. Its lack of pleochroism in thin section is probably due to its low iron content.

OTHER OCCURRENCES OF EPITHERMAL HÜBNERITE.

A review of the literature indicates that the presence of hübnerite in epithermal veins is not rare. It was found in minor amounts in the lower levels of the Puzzle vein at Cripple Creek, Colorado.[8] Hess[9] described hübnerite from veins in the deeper workings of the mines at Tonopah, Nevada. Here it occurred as thin platey red to dark red crystals. Other instances are cited by Lovering[10] where hübnerite is associated with other mineral assemblages that indicate its deposition in the epithermal zone. The occurrence of hübnerite with other manganese minerals in gold and silver epithermal deposits has a genetic significance. In discussing the genesis of certain manganese minerals, occurring

[6] Director, Nevada State Analytical Laboratory.

[7] Hess, F. L.: Tungsten minerals and deposits. U. S. Geol. Surv. Bull. 652, p. 22, 1917.

[8] Lindgren, Waldemar, and Ransome, F. L.: Geology and gold deposits of the Cripple Creek district, Colorado. U. S. Geol. Surv. Prof. Paper 54, p. 127, 1906.

[9] Hess, F. L.: *Op. cit.*, p. 26.

[10] Lovering, T. S.: Ore deposits of the western states. A. I. M. E., pp. 668–669, 1933.

with hübnerite, Hewett and Pardee [11] state that "the paragenesis and position of the manganiferous bodies indicate that they lie near the upper limit of the mesothermal zone if not beyond it." Where disseminated in fine grains, or where intimately intergrown with ore minerals, hübnerite may have an appearance not unlike that of sphalerite and therefore might readily escape detection. It is probable that this mineral is far more common to gold-silver veins than is at present realized.

ACKNOWLEDGMENTS.

Many courtesies were extended to the writer, during visits to the district, by Mr. N. H. Bennet, owner of the Zaca mine. To Professor William I. Smyth thanks are due for the chemical analysis. Mr. O. J. Benstron supplied specimens and gave much valuable information on the occurrences of hübnerite and Mr. A. L. Stewart called attention to the locality where the finest crystals were found and aided in collecting specimens.

MACKAY SCHOOL OF MINES,
UNIVERSITY OF NEVADA,
January 12, 1938.

[11] Hewett, D. F., and Pardee, J. T.: Ore deposits of the western states. A. I. M. E., p. 675, 1933.

DISCUSSION AND COMMUNICATIONS

STRUCTURAL ENVIRONMENT OF THE BENDIGO GOLDFIELD.

Sir:—Dr. J. B. Stone, in his article "The Structural Environment of the Bendigo Goldfield," published in ECONOMIC GEOLOGY, vol. 32, November, 1937, states that the "lava" dikes of Bendigo are lamprophyres, citing Dr. Stillwell's paper of 1912, in which they are described as monchiquites, accompanied by occasional camptonites.[1] These monchiquite dikes were tentatively correlated by Dr. Stillwell with Tertiary ultrabasic lava flows near Macedon, Victoria. Dr. Stone, however, attaches little weight to this correlation, and suggests that they are related to the lamprophyre dikes that "are found in all, or nearly all, of the Victorian goldfields," in association with Devonian granitic rocks. He thinks that "the scanty direct evidence of the age of the lamprophyre dikes of Bendigo agrees as well with a Devonian as with a Tertiary age, and that the close association of dikes and quartz veins is more than a coincidence."

This interpretation of the Bendigo monchiquite dike is at variance not only with a suggestion of direct evidence as to age at Kangaroo Gully, near Bendigo, but also with important analogous evidence in other parts of Victoria, developed since the publication of Stillwell's descriptions in 1912.

The direct evidence is to be found at Kangaroo Gully, four and a half miles southwest of the city of Bendigo, where an outcrop of a monchiquite agglomerate occurs. The agglomerate, which is cut by a monchiquite dike, contains glaciated pebbles. This peculiar deposit, with which Dr. Stone is no doubt acquainted,

[1] Stillwell, F. L.: Preliminary notes on the monchiquite dykes of the Bendigo Goldfield. Roy. Soc. Victoria Proc., n. s. vol. 25, no. 1, pp. 2–14, 1912.

has been described by Professor E. W. Skeats,[2] and whatever view is held in regard to its origin, only one glacial period is known in Victoria, and that is of Permo-Carboniferous age. The inference is, therefore, that the monchiquite dikes are post Permo-Carboniferous.

The Devonian lamprophyres to which Dr. Stone ascribes similarity, are in the main minettes and spessartites, although in places, as in the Walhalla-Wood's Point district, to which Dr. Stone refers, they are accompanied by camptonites.[3] They generally occur in typical association with diorite-porphyrites, and are petrologically and genetically distinct from the monchiquites and the camptonites that accompany the monchiquites, a point that Dr. Stone has overlooked in his use of the more general name "lamprophyre."

The monchiquite dikes at Bendigo, on the other hand, are actually only part of a swarm of such dikes that extends southwards through Maldon, Castlemaine, Daylesford, Steiglitz, Greendale, and Yaloak, to the coast. At Greendale and at Korkuperrimul Creek, they are associated with olivine-basalts, olivine-nephelinites, and camptonites. In several instances at Korkuperrimul Creek, monchiquite dikes can be observed to pass up into lava flows of limburgite (identical in appearance under the microscope) that are intercalated between flows of basalt of the Tertiary Older Volcanic Series of Victoria.[4] At Gorong similar dikes intrude Devonian granites and some Older Volcanic basalts, but underlie the Newer Volcanic basalts. Identical monchiquite dikes occur in South Gippsland, as members of a strong dike swarm that includes crinanites and olivine-basalts.[5] These dikes intrude the Jurassic sediments of the region but underlie Tertiary sediments, and belong to the Tertiary Older Volcanic Series. In

[2] Skeats, E. W.: On a volcanic agglomerate containing glaciated pebbles at Kangaroo Gully, near Bendigo. Roy. Soc. Victoria Proc., n. s. vol. 26, no. 2, pp. 373–385, 1914.

[3] Junner, N. R.: The geology and ore deposits of the Walhalla-Wood's Point auriferous belt. Aust. Inst. Min. Met. Proc., n. s., no. 39, pp. 162–166, 1920.

[4] Jacobson, R., and Scott, T. R.: Geology of the Korkupperimul Creek area, Bacchus Marsh. Roy. Soc. Victoria Proc., n. s. vol. 50, no. 1, 1937.

[5] Edwards, A. B.: Tertiary dykes and volcanic necks of South Gippsland. Roy. Soc. Victoria Proc., n. s. vol. 47, no. 1, pp. 112–134.

East Gippsland, still more monchiquite dikes occur accompanying olivine-basalts, crinanites, phonolites, occasional camptonites, and olivine-nephelinite (nepheline-monchiquite). In their association, as well as petrologically, these monchiquites resemble the monchiquite dikes of the North-west Dyke Swarms of Scotland.

The evidence for the Tertiary age of the Bendigo dike is, therefore, much stronger than Dr. Stone is inclined to believe. It is highly unlikely that they have any genetic relationship to the quartz reefs. Their close association in the field is probably due to a re-opening of pre-existing lines of weakness during the early Tertiary. This being so, as Dr. Stone remarks on p. 881, "This period of tension naturally loses any significance in connection with the ore deposits."

A. B. EDWARDS.

GEOLOGY DEPARTMENT,
UNIVERSITY OF MELBOURNE,
Jan. 3, 1938.

DERBY ON THE GENESIS OF THE DIAMOND.

Sir:—Doctor Orville A. Derby's opinion carried much weight among his colleagues, who respected him for his modesty and his cautious statements. He was a searcher after truth, and very tenacious in defense of a seemingly well founded proposition until convinced of error or uncertainty in his data or by the weight of contrary evidence disproving his premises.

His hypothesis as to the origin of the diamond was accordingly favorably received by many until he himself expressed doubt on account of the meagerness of material for its support and because of the well proven mode of occurrence in basic eruptives in Africa and elsewhere.

The idea that the diamond originated in the rocks where it is found is very old, having been held since olden times in India and other countries. In Brazil the occurrence and distribution of the diamond was studied by Baron Eschwege over a hundred years ago as well as by Mawe, the English mineralogist, who traveled extensively in this country and gives in his book much information on the subject during colonial times. This was written be-

fore the establishment of an independent government in Brazil in the year 1822. After the founding of the School of Mines at Ouro Preto in 1870, its first director, Professor Gorceix, made many critical studies of the same nature on this subject. In the latter years of the Empire, Dr. Derby at that time, Director of the São Paulo Geological Survey, examined various localities in the Diamantina district and formulated a hypothesis as to the origin of the diamond which has been widely quoted in technical and scientific literature.

With the passage of time, however, and the accumulation of material bearing on the subject, notwithstanding his very suggestive observations at São Joao de Chapada, Derby's faith in the validity of this hypothesis began to waver and in 1911, as Director of the Brazilian Geological Survey, his more mature opinion on this interesting subject was expressed in an article in the *Journal of Geology,* (No. VII, p. 627), where he wrote as follows: "Dr. Wagner's summary of what is known of the geological conditions in which diamonds occur in South Africa, suggests certain speculative points of view, which if found worthy of attention, may in turn suggest desirable lines of investigation in the field and laboratory. These inquiries may perchance throw light on the intricate and fascinating question of the genesis of the diamond." He states further: ". . . until such *prospecting,* as suggested, is done on a sufficiently large and efficient scale in the old crystalline areas outside of existing diamond fields, to prove the *existence* or *non-existence* of ultra-basic olivine-bearing eruptives, the opinion that here the mode of occurrence is essentially different from that observed in Africa, should be held in suspense."

Toward the end of 1914, a year before his tragic death, Derby had already become quite convinced that this mode of occurrence holds true in Brazil as well. He had only recently learned of the great extent and thickness of the basaltic flows in the Serra da Matta de Corda, many samples of which had been brought down some years before by the late Dr. Francisco de Paula Oliveira, father of our esteemed Director, and which were studied by Derby and Hussak.

At about the same time, Derby had learned of the existence of the immense beds of volcanic ash found spread over vast areas of this region of the Serra da Matta de Corda, and farther south, where in many places it is over sixty meters thick, contains numerous volcanic bombs, and nodules. Also still farther west, he learned of an old Paleozoic volcano, whose cone is largely eroded and is covered by lateritic material. This volcano seemed to be a possible locus of origin of the diamond because the streams heading thereabouts are all diamond-bearing, as are also most of those of the Serra da Matta de Corda. (The existence of many dikes and bosses of trachytic eruptive in the vicinity of this old volcano is of special interest in this connection as being rocks of an acidic nature which may or may not have some relation to the origin of the diamond, and which offer an interesting field for study; they may at least, furnish some index as to the character of this old volcano which, as yet, is somewhat obscure).

At about this time also, he received samples of peridotite from the Jacobina region of Bahia, and specimens of melilite-basalt from Joazeiro, farther north in the same state. These facts, together with many others bearing on the subject, convinced him that the gems of the wide spread diamond fields of Brazil may have originated in such basic rocks, and that, with further mapping on a large scale, making possible intensive and thorough geological studies in the field, such pipes and such occurrences might eventually be found in this country.

All these facts came out in the discussions that Derby had with various friends regarding the subject generally, and particularly in regard to the then lately received book by the late Dr. Percy Wagner on *The Diamond Fields of Southern Africa,* an epitome of the known facts regarding the diamond and its origin.

It will be a satisfaction to the many friends who still cherish his memory, to know that Derby's conclusions were well considered and that, in all probability, if his suggestions were followed out, convincing results would soon appear.

Only a few years after his death, under the able direction of Drs. Gonzaga de Campos and Euzebio de Oliveira, geologists of the Survey found many pipes of this eruptive in the Serra da

Matta de Corda, some of which have been tried out in a small way for diamonds but as yet without success. Studies are being continued in this region at the present time.

Again, some ten years later, an occurrence of peridotite was found in the crystalline complex, some fifty miles east of the city of Lençóes, the famous diamond and carbon center of Bahia, which may prove to be a pipe when properly examined. This could not be done at the time of its discovery because of its being in the form of a wide pan and full of water at the time, forming a shallow pond. When this pond dries up after the rainy season, such examination should easily be made. This occurrence may well be what we are looking for!

The many occurrences of peridotite in the form of pipes and dikes, together with chrome-ore which has come to light in the Jácobina region in recent years, are also very suggestive in this connection, especially in this region, since observations in the field show that the diamond-bearing beds of the Chapada Diamantina of Bahia are of fluviatile origin and that they were deposited by a stream that was flowing from the north toward the south.

On the other hand, scientists of the Department of Mines, namely, Drs. Djalma Guimarães and Luciano Jacques de Moraes, have followed up the ideas Derby presented and maintained, suggesting a mode of occurrence of the diamond in an acid medium. Owing principally to the persistent studies of the first of these gentlemen, both in the field and in the laboratory, they have found many diamonds with quartz inclusions and in such environment that they became convinced of the correctness of Derby's original hypothesis. They have given extended notice of these studies in various publications in this country and also in an article in *Economic Geology* (Vol. 26, No. 5, 1931).

HORACE E. WILLIAMS
Assistant Chief (Retired) of the Brazilian Geological Survey.

235 RUA COMM. BASTOS,
ILHA GOVERNADOR,
RIO DE JANEIRO.

REVIEWS

Maryland Geological Survey, volume 13. Pp. 295; figs. 36; pls. 46. Johns Hopkins Press, Baltimore, Md., 1937. Price, $1.25. Pt. 1: E. Cloos, The application of recent structural methods in the interpretation of the crystalline rocks of Maryland, pp. 29–105. Pt. 2: H. G. Hershey, Structure and age of the Port Deposit granodiorite complex, pp. 109–148. Pt. 3: C. H. Broedel, The structure of the gneiss domes near Baltimore, Maryland, pp. 151–187. Pt. 4: J. Marshall, The structure and age of the volcanic complex of Cecil County, Maryland, pp. 191–213. Pt. 5: C. J. Cohen, Structure of the metamorphosed gabbro complex at Baltimore, Maryland, pp. 217–236. Pt. 6: C. W. Carter, The Upper Cretaceous deposits of the Chesapeake and Delaware canal of Maryland and Delaware, pp. 239–295.

Students of crystalline rocks and their structural problems, especially in the Appalachian piedmont, will find much new information in this book, which is the result of recent field work by geologists of the Maryland Geological Survey. In the sixth part, new data are given on the stratigraphy of the Upper Cretaceous rocks in the Coastal Plain.

In part 1, Ernst Cloos who supervised the field work on the crystalline rocks, summarizes field methods useful in surveying these rocks, with emphasis on megascopically visible structures, but including an interesting example of rock fabric analysis in schist near the Martic overthrust; a bibliography of 120 titles is appended. In parts 2–5, these methods are applied to the structure of the Baltimore gneiss, the crystalline schists of the younger Glenarm series, and to intrusions of granodiorite and gabbro that are younger than the Glenarm rocks.

According to Broedel (part 3), the internal structure of most of the Baltimore gneiss blocks northwest of Baltimore is essentially pre-Glenarm; even the domical form of some of them seems to go back to an older anticlinal structure of the flowage elements. Many structure maps allow a close check on the author's conclusions.

Between some of these gneiss blocks the Baltimore gabbro was intruded. Contacts, planar and linear structure elements of this mass, as shown by Cohen (part 5), indicate a funnel-like shape with a steep western border. The general northeast pitch of the lineation suggests that the principal lengthening was at a high angle upward to the southwest, but

with local variations in direction. The author describes "secondary" structures which have been superimposed on "primary" ones, but as he uses a great many different terms (over 13 on pp. 233–235) without defining each explicitly, it is very difficult to understand how many secondary structures there are, and just how late each one developed. At least some of them are held to be due to great "intensity of folding," or to "regional metamorphism." The use of the latter term without explicit definition, but in contradistinction to deformation due to intrusions, is inadvisable as no two persons are likely to hold the same views with regard to the agencies that produced regional metamorphism. (See H. H. Read: Metamorphic correlation in the polymetamorphic rocks of the Valla Field block, Unst, Shetland Islands, Trans. Royal Soc. Edinb., vol. 59, pt. 1, p. 199, 1937.)

A structural study of the Port Deposit granodiorite in northeastern Maryland by Hershey (part 2) has furnished interesting evidence for a post-Lower Ordovician age of this and other intrusions, including the Baltimore gabbro. The granodiorite encloses fragments of argillaceous sediments resembling the Peters Creek schist. Both the schist and the early Ordovician Conestoga limestone are traversed by what appears to be the same fracture cleavage, and this structure is preserved in the cores of some xenoliths, whereas the contact metamorphism has blurred the structure along their edges. The authors assume, therefore, that the intrusion is younger than the cleavage which, in turn, is post-Conestoga. If this is the true age of the intrusions, it would throw important light on Keith's thesis that the westward bulges in the folded Appalachians are opposite large intrusive centers.

In part 4, Marshall describes the structure and petrography of a roof pendant in the Port Deposit granite. The tightly folded complex of volcanic and intrusive rocks is presumably of early Glenarm age.

New cuts in the Chesapeake and Delaware canal, studied by Carter (part 6), allow a subdivision of the Upper Cretaceous rocks into the Crosswicks clay (at base), Englishtown sand, and Marshalltown formation. Correlation and distribution of several other formations are discussed.

The numerous observations gathered by the authors of the structural papers, are made readily available through 10 large structure maps and many excellent photos, line drawings, charts, and tables. The work shows how many important new data can be brought to light through detailed studies, in a region of poor outcrops, where much areal mapping and petrographic work has already been done. The reviewer understands that the decision to publish this volume was reached rather late in the year by the Maryland Geological Survey, and as the editorial work had to be cut short, some typographical and stylistic errors have remained in

the text. The nominal cost and the variety of problems discussed make this a very useful and stimulating book.

R. BALK.

The Geology and Biology of the San Carlos Mountains, Tamaulipas, Mexico. By L. B. KELLUM AND OTHERS. Pp. xi + 341, Pl. 30; Figs., 18; Maps, 13. Univ. of Michigan Press, 1937. Price $5.00.

This handsome volume consists of 9 parts covering the geology and biology of the San Carlos Mountains in the northeast corner of Mexico, about 80 miles south of the Rio Grande River. The area was first examined for the Standard Oil Company of New Jersey in 1925. In 1930 the work done in 1925 was released for publication and upon this was based a more thorough survey with the support of the University of Michigan and the National Research Council. The result is recorded in the present volume. The map made in 1925 was revised, and the geology and the biology of the district was discussed by 9 scientists in 5 chapters on the biology of the region and 4 chapters on its geology.

The geology chapters are entitled: (1): Geology of the Sedimentary Rocks of the San Carlos Mts., by L. B. Kellum, (2) Igneous Rocks of the San Carlos Mountains, by E. H. Watson, (3) Ore Deposits of the San Carlos Mts., by E. S. Bastin, and (4) Geology of the Sierra de Cruillas, by R. W. Imlay.

According to Kellum by far the greater part of the sedimentary rocks involved in the folded San Carlos Mountains are Lower and Upper Cretaceous, though he realizes that other geologists have reported the existence in small masses of older sediments, which on paleontologic evidence they regarded as Jurassic; and surrounding the Cretaceous beds are deposits of Eocene shales. All are gently folded with E.–W. axes and are domed locally around intrusions of laccolitic and sill forms. These intrusions consist of an older group of calc-alkalic rocks, ranging between gabbros and diorites and mikrogranites and alaskites occurring mainly as laccolites, and a younger group in the forms mainly of dikes, sills and plugs, though the largest intrusion in the mountains, which has the form of a laccolite is a "complex" of alkalic rocks. The alkali rocks are so varied in composition that the author does not give them specific names, though there are among them darker ones, such as essexites, camptonites, kersantites and shonkinites and among the lighter ones alkali syenites, monzonites, bostonites and foyaites. In a few valleys around the mountains are flows of olivine basalt and beds of basaltic agglomerates.

Bastin's studies indicate that the copper deposits of the San José district are small, irregularly shaped contact metamorphic deposits near the contacts of limestone with the dioritic intrusion that lies just north of the alkalic rock "complex" mainly within the igneous rock. The "mineral-

izing solutions are conceived to have been igneous solutions of calcium bicarbonate carrying moderate amounts of iron, sulphur and copper but only small amounts of silica." The lead-silver deposits of San Nicolus, El Palmar and Moctezuma, on the other hand, are fissure veins, and, subordinately replacements in limestone. They consist mainly of calcite and galena, containing silver and occasionally a little zinc. Their scanty gangues, in addition to calcite, are composed of small amounts of siderite, barite, pyrite and sphalerite. They are supposed to have been deposited from magmatic solutions but at some distance from the igneous source and under moderate or low temperature conditions.

In the Sierra de Cruillas the most common igneous rock is a microgranite appearing in sills and less commonly in dikes, but there occur also vogesites, trachytes and various olivine and alkali basalts. These are all intruded into Lower Cretaceous (Tamaulipas) limestone producing contact metamorphism.

The structures of the areas studied are described in fair detail, as are also the mineralogical character and texture of the numerous igneous rocks encountered.

The book is well illustrated and is furnished with a comprehensive index.

W. S. BAYLEY.

Our Wandering Continents. By ALEX. L. DU TOIT. Pp. xiii, 366; figs. 48. Oliver and Boyd, Edinburgh, 1937. Price, 18 shillings.

The author of this stimulating book has been for years an active proponent of the continental drift hypothesis. He has travelled extensively in several continents to examine critical field evidence bearing on the concept, and has published several well-known articles in which he presents his evidence and arguments. The present work, dedicated to the late Alfred Wegener, assembles a vast amount of information, largely stratigraphic and structural, which he has sifted from publications in many lands. In his opinion, it is not possible to interpret all of the geologic facts intelligibly without recognizing continental drift on a large scale.

Du Toit's synthesis differs considerably from Wegener's. In the first place, he assumes, in accord with the views of most geophysicists, that the Earth's axis of rotation has remained fixed. During the Paleozoic, the present land masses, South America, Africa, Arabia, India, Australia, and Antarctica were joined in one vast continent, Gondwana, with its center in high southern latitudes. This hypothetical reassembly of lands now widely separated solves the problem of the extensive late Paleozoic glaciation by assuming a single vast ice cap moving radially from the vicinity of the South Pole; it is offered also as the explanation of paleon-

tologic and lithologic affinities between the several land units. The author presents geologic evidence bearing on the former existence also of a great northern continent, Laurasia, which has been dismembered to form North America, Greenland, and Eurasia.

In an attempt to explain the mechanics of continental drift, Du Toit includes in an eclectic hypothesis the centrifugal force of rotation, convection currents beneath the continental blocks, "deleveling" of the crust, and other earlier suggestions. He adds a concept of his own, referred to as the "paramorphic principle," according to which the minerals that are stable in the outer part of the crust suffer change, below a critical depth, to other minerals of higher density and smaller specific volume; a change assumed to be reversible on return of the material to the higher zone. Du Toit arrives at the startling conclusion that erosion of a given thickness of rock may result in uplift of the surface far beyond its original altitude, by combination of isostatic rise and paramorphic expansion. However, analysis of the calculation shows that the value derived for the expansion is about four times too great, under the assumed conditions (p. 239). The concept of a paramorphic zone is attractive; but there is grave doubt whether inversion from the dense to less dense minerals occurs on the scale assumed, without fusion. For example the diamond, one of the minerals listed as characteristic of the paramorphic zone, certainly is stable at low pressures.

Du Toit's book has the large merit that it attempts to evaluate all available geologic evidence relating to the question of continental drift. Only by persistent and thorough use of this method can we hope eventually to give the hypothesis a satisfactory test. In the meantime, it is safe to say that many geologists whom Du Toit would class as "orthodox" are not nearly as hostile to the hypothesis as he seems to think. No thoughtful person can ignore some of the geologic arguments in favor of drift, such as, for example, the peculiar distribution of late Paleozoic glaciation. In the opinion of the reviewer, Du Toit has not yet proved his case; but he has helped it, and his effort is praiseworthy.

CHESTER R. LONGWELL.

Professor David. The Life of Sir Edgeworth David. By M. E. DAVID. Pp. 320; Pl. 8; map. Edward Arnold & Co., London, 1937. Price 12s. 6d.

This biography of Sir Edgeworth David is a matter-of-fact record of the life of a man who was so closely connected with the development of the geology of Australia that his name will always be recalled when the geology of his country is being discussed. As a teacher at the University of Sydney he was admired and almost revered by his students and by his

colleagues, but it is mainly through his investigations on the ancient glacial deposits in Australia, through the borings supervised by him at the atoll of Funafuti, in the hope of settling the controversy between the conflicting theories as to the formation of coral atolls, through his discovery with two companions of the site of the South Magnetic Pole and his work as a mining engineer on the Western Front during the World War, and through the great geological map of Australia made by him and published only 2 years before his death, that he is best known to the geological world at large.

The account of his life is given in great detail. It is based largely upon Sir David's diaries and upon the accounts of his work described in newspapers and in the records of geological societies. Those parts dealing with the discovery of the South Magnetic Pole and with his activities during the war are particularly interesting, mainly because they consist largely of quotations from Sir David's own letters and reports. Indeed the volume contains so many quotations from Sir David's own writings that in its major features it is almost an autobiography. It is well worth reading.

W. S. BAYLEY.

Outlines of Historical Geology, 3rd Edit. By CHARLES SCHUCHERT AND C. O. DUNBAR. Pp. 241; figs. 151. John Wiley & Sons, New York, 1937. Price $2.50.

This book is entirely rewritten, frankly for the student who wishes to obtain a survey of historical geology in one brief course. It departs from the period by period treatment. Part I, The Nature of the Evidence, treats of the Living Record of the Dead, the Record in the Rocks and Geologic Time. Part II, Earth's Changing Features, is a brief survey from pre-Cambrian to Recent. Part III, the Pageant of Life, deals with Paleozoic Life, Reign of Reptiles, and Age of Mammals. Part IV is The Coming of Man.

The volume is well organized and the departure from former hidebound order of treatment makes it more interesting. It contains an excellent choice of illustrations. The interested layman as well as the student will benefit from reading it.

Descriptive Petrography of the Igneous Rocks. Vol. III. By ALBERT JOHANNSEN. Pp. 360; figs. 178. Univ. of Chicago Press, Chicago, 1937. Price $4.50.

Volume III of this monumental work follows Volume I, on the textures, structures, and classification of igneous rocks, and Volume II, on the quartz-bearing rocks. This volume on the Intermediate Rocks, deals

with the syenite-diorite-gabbro series and their extrusive and hypabyssal equivalents. As before, the rocks are considered under Families (9–12) and Orders, and there are likewise many good photomicrographs and photos of petrographens. Like the preceding volume, it is complete in description, analyses, classification and historical background.

Les Ressources minérales de la France d'outre-Mer. Vol. 5, Le Pétrole. Pp. 263; figs. 37. Bur. d'Études Géol. et Min. Col. Paris, 1937. Price 45 francs.

This, the 5th volume in the series, follows those in Coal, Iron, Metals, and Phosphate. The various chapters are written by Messrs. H. de Cizancourt, L. Migaux, L. Dubertret, F. Blondel, and J. Filhol. The first chapter is a miniature text book on the character, origin, migration, accumulation, and occurrence of petroleum. Methods of search are taken up in another chapter. The other chapters deal with petroleum in Morocco, Algeria, Tunisia, French Equatorial Africa, Madagascar, Syria, Irak, and other colonies.

The volume brings together for the first time full information regarding petroleum in all French lands.

BOOKS RECEIVED.

J. D. BATEMAN.

Geology of Highland County, Ohio. J. K. ROGERS. Pp. 148; fig.; map. Ohio Geol. Surv., Bull. 38 (4th ser.). Columbus, 1936. *Glacial geology; stratigraphy; oil and gas.*

Analyses of Colorado Coals. Pp. 327; figs. 2. U. S. Bur. Mines, Tech. Paper 574, 1937. Price, 25 cts.

Preliminary Report on Gypsum Deposits of Bay St. George, Southwestern Newfoundland. O. A. HAYES AND H. JOHNSON. Pp. 18; figs. 10; map. Nfld. Dept. Nat. Res., Geol. Sect., Inform. Circ. 3. St. John's, 1937. *Accessible gypsum deposits with thicknesses up to 300 feet.*

O Cretaceo De Sergipe. CARLOTTA J. MAURY. Pp. 283. Serv. Geol. E Miner., Mon., vol. 11. Rio de Janeiro, 1936. *A valuable contribution to correlations of Brazilian Cretaceous formations with those of Venezuela, Colombia, Peru, Ecuador, Mexico, and Texas; descriptions of vertebrate and invertebrate fauna; comprehensive but lacks illustrations; written in English and Spanish on alternate pages.*

Origin of the Magnetite Deposits at Lyon Mountain, New York. DAVID GALLAGHER. Pp. 85; figs. 17. N. Y. State Mus., Bull. 311. Albany, 1937. *Mineralization ascribed to pneumatolytic metasomatism.*

Clay. G. I. WHITLATCH. Pp. 35. Tenn. Dept. of Conserv., Div. of Geol., Markets Circ. 6. Nashville, 1937.

Geology and Fuel Resources of the Southern Part of the Oklahoma Coal Field. Part 2: The Lehigh District, Coal, Atoka, and Pittsburgh Counties. M. M. KNECHTEL. Pp. 58; figs. 2: map. U. S. Geol. Surv. Bull. 874-B, 1937. Price, 20 cts. *Exposed Carboniferous and unexposed Cambrian to Carboniferous strata.*

Bibliography and Index of West Virginia Geology and Natural Resources to July 1, 1937. J. B. LUCKE. Pp. 84. W. Va. Geol. Surv. Morgantown, 1937.

Coke from Illinois Coals. G. THIESSEN. Pp. 235; figs. 82. Ill. Geol. Surv., Bull. 64. Urbana, 1937. *Coke suitable for domestic and some industrial fuel can be made from most coals occurring in Illinois.*

Coal Deposits of Pike County, Kentucky. C. B. HUNT, G. H. BRIGGS, JR., A. C. MUNYAN, AND G. R. WESLEY. Pp. 92; pls. 2; fig.; maps, 48. U. S. Geol. Surv. Bull. 876, 1937. Price, $2.25 (including maps). *Reserves of 8 billion tons of coal in minable beds; detailed sections.*

Stratigraphic Studies of Pennsylvanian Outcrops in Part of Southeastern Illinois. W. A. NEWTON AND J. M. WELLER. Pp. 31; figs. 12; map. Ill. Geol. Surv., Rept. of Invest. 45. Urbana, 1937. *Pennsylvanian formations are correlated on the basis of "cyclothems" (depositional cycles).*

The Mica in Argillaceous Sediments. R. E. GRIMM, H. R. BRAY, AND W. F. BRADLEY. Pp. 17. Ill. Geol. Surv., Rept. of Invest. 44. Urbana, 1937. *"Illite" (Illinois) is proposed as a new name for a mica mineral resembling muscovite and sericite but with distinctive properties.*

Illinois Basin. J. M. WELLER AND A. H. BELL. Pp. 18; figs. 6. Ill. Petrol., no. 30. Urbana, July 3, 1937. *Three new oil wells, two of them in the deep basin area, have been located on basis of seismograph surveys.*

Mineral Resources of Kansas Counties. K. K. LANDES. Pp. 110. Kan. Geol. Surv., Min. Res. Circ. 6. Lawrence, June 1, 1937. *Kansas ranks first among states in volcanic ash production, third in zinc, and fourth in salt and petroleum.*

Contributions to Virginia Geology. Pp. 211; figs. 10; pls. 22. Va. Geol. Surv. Bull. 46. Richmond, 1936. *Collection of geological essays and results of investigations.*

Geology and Ground-Water Resources of Duval County, Texas. A. N. SAYRE. Pp. 116; figs. 3; pls. 7; map. U. S. Geol. Surv., W-S Paper 776, 1937. Price, 25 cts. *Little possibility of irrigation with well water.*

The Floods of March, 1936. Part 1: New England Rivers. Pp. 466; figs. 41; pls. 14. U. S. Geol. Surv., W-S Paper 798, 1937. Price, 70 cts. *Floods due to concentrated precipitation in conjunction with melting snow and ice; considerable statistical data.*

Texas Through 250,000,000 Years. Pp. 30; figs. 17. Humble Oil and Refining Co., Houston, 1937. *An interesting descriptive booklet telling the story of oil and geology in Texas.*

Structural Materials of the Tennessee Valley Authority Region. Pp. 25. T.V.A., Div. of Geol., Bull. 6. Knoxville, 1937. *Limestone, marble, sandstone, crushed stone, sand, gravel, cement, lime, and gypsum.*

El Oro en el Peru en 1936. J. HOHAGEN. Pp. 156; figs. 4. Cuerpo de Ing. de Minas del Peru, Bol. 118. Lima, 1937. *History, production, and economics of gold mining in Peru.*

Bibliography of United States Bureau of Mines Investigations on Coal and Its Products, 1910–35. A. C. FIELDNER, A. H. EMERY, AND M. W. VON BERNEWITZ. Pp. 145. U. S. Bur. Mines, Tech. Paper 576, 1937. Price, 15 cts.

Aids to the Student of Conservation. S. S. VISHER. Pp. 32. John Wiley & Sons, New York, 1937. Price, 25 cents. *A series of questions called* "Review" *and* "Thought Provoking Questions" *concerning each chapter of the book,* Our Natural Resources and their Conservation, *edited by Parkins and Whitaker.*

Carte du Katanga. Belgian Congo, map of Sakabinda. Scale 1:200,000; 23 × 32 in.; colors; with explanatory booklet, 48 pp. Com. Spec. du Katanga, Brussels, 1937.

Das Erdöl. und seine Verwandten in den Philippinen. K. A. F. R. MUSPER. Pp. 16. De Ing. in Ned.-Indië, vol. 4, no. 8, Aug. 1937, pp. 141–157. Batavia. *Occurrence, possibilities, physical, and chemical properties of oil in the various Philippine Islands.*

The State of Geologic Research in the Oil Industry. D. C. BARTON. Pp. 10. A. A. P. G. Bull., vol. 21, no. 5, 1937. *A thoughtful study of what has been and should be done.*

El Oro en el Peru en 1936. J. HOHAGEN. Pp. 156. C. de Ing. de Minas, Bull. 118, Lima, 1937. *History of gold mining, production, costs, and mines.*

Type Invertebrate Fossils of North America. (Devonian) Beyrichiacea. Compiled by A. S. WARTHIN, JR. 150 cards 8.5 × 11. Warner Free Inst. of Science. Philadelphia, Pa., 1937. *Each card contains illustrations, type data, occurrence, and descriptions of individual species.*

Financial Post Survey of Mines. 11th Ann. Edition, 1937–38. Pp. 360; maps. McLean Publishing Co., Toronto, 1937 (Nov.). Price, $2.00. *Analysis of 2,100 companies producing 400 minerals in Canada and Newfoundland, giving location, development, production, management, and financial status; also mineral production by minerals and provinces; 21 maps of mineral areas in a special section.*

South Africa Geological Survey, Memoir 33, Geology of Bethlehem and Kestell and Oil Indications. (With geol. map, 21 × 34, scale 1:125,000.) O. R. VAN EEDEN. Pp. 68; figs. 3. Price, 7/6. *Stratigraphy, non-metallics, and petroleum indications and possibilities; standard areal map.* **Geology of the Coastal Belt near Gamtoos Valley, Cape Prov.** S. H. HAUGHTON, H. F. FROMMURZE, AND D. J. L. VISSER. Pp. 64; pls. 3; 2 geol. sheets 31 × 34, scale 1:148,752. Price 5 s. *Pre-Cambrian to Recent systems; Minor precious and base metals.* Pretoria, 1937.

SCIENTIFIC NOTES AND NEWS

H. F. HARWOOD has been awarded the Wollaston Fund by the Geological Society of London, "in recognition of his research work on the chemistry of minerals and rocks."

B. V. LOMBAARD has been elected president of the Geological Society of S. Africa.

WALDEMAR LINDGREN will relinquish the chairmanship of the *Annotated Bibliography of Economic Geology* in July of this year, upon the completion of the tenth volume of this publication which he founded and guided so well during this decade. He will, however, continue to serve on the Board and Staff. A 10-volume index of the Bibliography will be prepared and distributed at the end of this year.

A. W. ROGERS has recently been awarded the degree of Doctor of Science by the University of Witwatersrand.

G. A. F. MOLENGRAAFF has received the Draper Memorial Medal from the Geological Society of S. Africa.

FREDERICK G. CLAPP, LESTER S. THOMPSON, HUBERT G. SCHENCK, and HENRY HOTCHKISS are heading parties engaged in geological surveying in southeastern Iran, with offices at Zahidan, Makran. Clapp drove from the fields of Inland Exploration Company in Afghanistan, to supervise this work. Thompson is in local charge for Amiranian Oil Company in this part of Iran. Schenck is conducting paleontological researches of the Lower Tertiary and Upper Cretaceous. Hotchkiss, until recently assisted by H. A. Afshar, is making detailed surveys on some of the attractive structures. E. Gardner Clapp is employed on some of the technical and administrative phases of the surveys, at times in Zahidan.

A. W. JOLLIFFE and G. S. HUME, of the Geological Survey of Canada, delivered a series of six lectures at Queen's University, Kingston, Ontario, during February and March. Dr. Jolliffe dealt with the mineral deposits of the North West Territories, and Dr. Hume with the new oil field in southern Alberta.

A. N. WINCHELL, of the University of Wisconsin, delivered the following Grant Memorial Lectures in March and April, at Northwestern University: (1) Where Art and Science Meet, (2) The Nature of Crystal Structure, (3) Isomorphism, (4) Optic Properties of Isomorphic Systems.

FRANCIS BAKER LANEY died of heart failure at Moscow, Idaho, on April 25, at the age of 63.

The 10th INTERNATIONAL PETROLEUM EXPOSITION will be held in Tulsa, Oklahoma, May 14–21, at which new geological exhibits and equipment will be shown.

The 2d GEOLOGICAL CONGRESS OF VENEZUELA was held in March at San Cristóbal with local excursions and a symposium on "The Geology of Venezuela."

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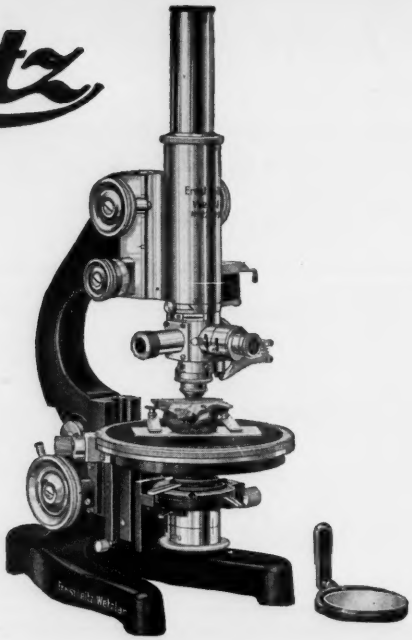

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